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SCIENCE AND HYPOTHESIS

SCIENCE AND HYPOTHESIS

BY

H. POINCARÉ

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AND AN INTRODUCTION BY

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AUTHOR'S PREFACE TO THE TRANSLATION.

I AM exceedingly grateful to Dr. Halsted, who has been so good as to present my book to American readers in a translation, clear and faithful.

Every one knows that this savant has already taken the trouble to translate many European treatises and thus has powerfully contributed to make the new continent understand the thought of the old.

Some people love to repeat that Anglo-Saxons have not the same way of thinking as the Latins or as the Germans; that they have quite another way of understanding mathematics or of understanding physics; that this way seems to them superior to all others; that they feel no need of changing it, nor even of knowing the ways of other peoples.

In that they would beyond question be wrong, but I do not believe that is true, or, at least, that is true no longer. For some time the English and Americans have been devoting themselves much more than formerly to the better understanding of what is thought and said on the continent of Europe.

To be sure, each people will preserve its characteristic genius, and it would be a pity if it were otherwise, supposing such a thing possible. If the Anglo-Saxons wished to become Latins, they would never be more than bad Latins; just as the French, in seeking to imitate them, could turn out only pretty poor Anglo-Saxons.

And then the English and Americans have made scientific conquests they alone could have made; they will make still more of which others would be incapable. It would therefore be deplorable if there were no longer Anglo-Saxons.

But continentals have on their part done things an Englishman could not have done, so that there is no need either for wishing all the world Anglo-Saxon.

Each has his characteristic aptitudes, and these aptitudes should be diverse, else would the scientific concert resemble a quartet where every one wanted to play the violin.

And yet it is not bad for the violin to know what the violoncello is playing, and *vice versa*.

This it is that the English and Americans are comprehending more and more; and from this point of view the translations undertaken by Dr. Halsted are most opportune and timely.

Consider first what concerns the mathematical sciences. It is frequently said the English cultivate them only in view of their applications and even that they despise those who have other aims; that speculations too abstract repel them as savoring of metaphysic.

The English, even in mathematics, are to proceed always from the particular to the general, so that they would never have an idea of entering mathematics, as do many Germans, by the gate of the theory of aggregates. They are always to hold, so to speak, one foot in the world of the senses, and never burn the bridges keeping them in communication with reality. They thus are to be incapable of comprehending or at least of appreciating certain theories more interesting than utilitarian, such as the non-Euclidean geometries. According to that, the first two parts of this book, on number and space, should seem to them void of all substance and would only baffle them.

But that is not true. And first of all, are they such uncompromising realists as has been said? Are they absolutely refractory, I do not say to metaphysic, but at least to everything metaphysical?

Recall the name of Berkeley, born in Ireland doubtless, but immediately adopted by the English, who marked a natural and necessary stage in the development of English philosophy.

Is this not enough to show they are capable of making ascensions otherwise than in a captive balloon?

And to return to America, is not the *Monist* published at Chicago, that review which even to us seems bold and yet which finds readers?

And in mathematics? Do you think American geometers are concerned only about applications? Far from it. The part of the science they cultivate most devotedly is the theory of groups of substitutions, and under its most abstract form, the farthest removed from the practical.

Moreover Dr. Halsted gives regularly each year a review of all productions relative to the non-Euclidean geometry, and he has about him a public deeply interested in his work. He has initiated this public into the ideas of Hilbert, and he has even written an elementary treatise on 'Rational Geometry,' based on the principles of the renowned German savant.

To introduce this principle into teaching is surely this time to burn all bridges of reliance upon sensory intuition, and this is, I confess, a boldness which seems to me almost rashness.

The American public is therefore much better prepared than has been thought for investigating the origin of the notion of space.

Moreover, to analyze this concept is not to sacrifice reality to I know not what phantom. The geometric language is after all only a language. Space is only a word that we have believed a thing. What is the origin of this word and of other words also? What things do they hide? To ask this is permissible; to forbid it, would be, on the contrary, to be a dupe of words; it would be to adore a metaphysical idol, like savage peoples who prostrate themselves before a statue of wood without daring to take a look at what is within.

In the study of nature, the contrast between the Anglo-Saxon spirit and the Latin spirit is still greater.

The Latins seek in general to put their thought in mathematical form; the English prefer to express it by a material representation.

Both doubtless rely only on experience for knowing the world; when they happen to go beyond this, they consider their foreknowledge as only provisional, and they hasten to ask its definitive confirmation from nature herself.

But experience is not all, and the savant is not passive; he does not wait for the truth to come and find him, or for a chance meeting to bring him face to face with it. He must go to meet it, and it is for his thinking to reveal to him the way leading thither. For that there is need of an instrument; well, just there begins the difference—the instrument the Latins ordinarily choose is not that preferred by the Anglo-Saxons.

For a Latin, truth can be expressed only by equations; it

must obey laws simple, logical, symmetric and fitted to satisfy minds in love with mathematical elegance.

The Anglo-Saxon to depict a phenomenon will first be engrossed in making a *model*, and he will make it with common materials, such as our crude, unaided senses show us them. He also makes a hypothesis, he assumes implicitly that nature, in her finest elements, is the same as in the complicated aggregates which alone are within the reach of our senses. He concludes from the body to the atom.

Both therefore make hypotheses, and this indeed is necessary, since no scientist has ever been able to get on without them. The essential thing is never to make them unconsciously.

From this point of view again, it would be well for these two sorts of physicists to know something of each other; in studying the work of minds so unlike their own, they will immediately recognize that in this work there has been an accumulation of hypotheses.

Doubtless this will not suffice to make them comprehend that they on their part have made just as many; each sees the mote without seeing the beam; but by their criticisms they will warn their rivals, and it may be supposed these will not fail to render them the same service.

The English procedure often seems to us crude, the analogies they think they discover to us seem at times superficial; they are not sufficiently interlocked, not precise enough; they sometimes permit incoherences, contradictions in terms, which shock a geometric spirit and which the employment of the mathematical method would immediately have put in evidence. But most often it is, on the other hand, very fortunate that they have not perceived these contradictions; else would they have rejected their model and could not have deduced from it the brilliant results they have often made to come out of it.

And then these very contradictions, when they end by perceiving them, have the advantage of showing them the hypothetical character of their conceptions, whereas the mathematical method, by its apparent rigor and inflexible course, often inspires in us a confidence nothing warrants, and prevents our looking about us.

From another point of view, however, the two conceptions are

very unlike, and if all must be said, they are very unlike because of a common fault.

The English wish to make the world out of what we see, I mean what we see with the unaided eye, not the microscope, nor that still more subtle microscope, the human head guided by scientific induction.

The Latin wants to make it out of formulas, but these formulas are still the quintessenced expression of what we see. In a word, both would make the unknown out of the known, and their excuse is that there is no way of doing otherwise.

And yet is this legitimate, if the unknown be the simple and the known the complex?

Shall we not get of the simple a false idea, if we think it like the complex, or worse yet if we strive to make it out of elements which are themselves compounds?

Is not each great advance accomplished precisely the day some one has discovered under the complex aggregate shown by our senses something far more simple, not even resembling it—as when Newton replaced Kepler's three laws by the single law of gravitation, which was something simpler, equivalent, yet unlike?

One is justified in asking if we are not on the eve of just such a revolution or one even more important. Matter seems on the point of losing its mass, its solidest attribute, and resolving itself into electrons. Mechanics must then give place to a broader conception which will explain it, but which it will not explain.

So it was in vain the attempt was made in England to construct the ether by material models, or in France to apply to it the laws of dynamic.

The ether it is, the unknown, which explains matter, the known; matter is incapable of explaining the ether.

POINCARÉ.

INTRODUCTION.

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THE treatise of a master needs no commendation through the words of a mere learner. But, since my friend and former fellow student, the translator of this volume, has joined with another of my colleagues, Professor Cattell, in asking me to undertake the task of calling the attention of my fellow students to the importance and to the scope of M. Poincaré's volume, I accept the office, not as one competent to pass judgment upon the book, but simply as a learner, desirous to increase the number of those amongst us who are already interested in the type of researches to which M. Poincaré has so notably contributed.

I.

The branches of inquiry collectively known as the Philosophy of Science have undergone great changes since the appearance of Herbert Spencer's *First Principles*, that volume which a large part of the general public in this country used to regard as the representative compend of all modern wisdom relating to the foundations of scientific knowledge. The summary which M. Poincaré gives, at the outset of his own introduction to the present work, where he states the view which the 'superficial observer' takes of scientific truth, suggests, not indeed Spencer's own most characteristic theories, but something of the spirit in which many disciples of Spencer interpreting their master's formulas used to conceive the position which science occupies in dealing with experience. It was well known to them, indeed, that experience is a constant guide, and an inexhaustible source both of novel scientific results and of unsolved problems; but the fundamental Spencerian principles of science, such as 'the persistence of force,' the 'rhythm of motion' and the rest, were treated by Spencer himself as demonstrably objective, although indeed 'relative' truths, capable of being tested once for all by the 'inconceivability

of the opposite,' and certain to hold true for the whole 'knowable' universe. Thus, whether one dwelt upon the results of such a mathematical procedure as that to which M. Poincaré refers in his opening paragraphs, or whether, like Spencer himself, one applied the 'first principles' to regions of less exact science, this confidence that a certain orthodoxy regarding the principles of science was established forever was characteristic of the followers of the movement in question. Experience, lighted up by reason, seemed to them to have predetermined for all future time certain great theoretical results regarding the real constitution of the 'knowable' cosmos. Whoever doubted this doubted 'the verdict of science.'

Some of us well remember how, when Stallo's 'Principles and Theories of Modern Physics' first appeared, this sense of scientific orthodoxy was shocked amongst many of our American readers and teachers of science. I myself can recall to mind some highly authoritative reviews of that work in which the author was more or less sharply taken to task for his ignorant presumption in speaking with the freedom that he there used regarding such sacred possessions of humanity as the fundamental concepts of physics. That very book, however, has quite lately been translated into German as a valuable contribution to some of the most recent efforts to reconstitute a modern 'philosophy of nature.' And whatever may be otherwise thought of Stallo's critical methods, or of his results, there can be no doubt that, at the present moment, if his book were to appear for the first time, nobody would attempt to discredit the work merely on account of its disposition to be agnostic regarding the objective reality of the concepts of the kinetic theory of gases, or on account of its call for a logical rearrangement of the fundamental concepts of the theory of energy. We are no longer able so easily to know heretics at first sight.

For we now appear to stand in this position: The control of natural phenomena, which through the sciences, men have attained, grows daily vaster and more detailed, and in its details more assured. Phenomena men know and predict better than ever. But regarding the most general theories, and the most fundamental, of science, there is no longer any notable scien-

tific orthodoxy. Thus, as knowledge grows firmer and wider, conceptual construction becomes less rigid. The field of the theoretical philosophy of nature—yes the field of the logic of science—this whole region is to-day an open one. Whoever will work there must indeed accept the verdict of experience regarding what happens in the natural world. So far he is indeed bound. But he may undertake without hindrance from mere tradition the task of trying afresh to reduce what happens to conceptual unity. The circle-squarers and the inventors of devices for perpetual motion are indeed still as unwelcome in scientific company as they were in the days when scientific orthodoxy was more rigidly defined; but that is not because the foundations of geometry are now viewed as completely settled, beyond controversy, nor yet because the ‘persistence of force’ has been finally so defined as to make the ‘opposite inconceivable’ and the doctrine of energy beyond the reach of novel formulations. No, the circle-squarers and the inventors of devices for perpetual motion are to-day discredited, not because of any unorthodoxy of their general philosophy of nature, but because their views regarding special facts and processes stand in conflict with certain equally special results of science which themselves admit of very various general theoretical interpretations. Certain properties of the irrational number π are known, in sufficient multitude to justify the mathematician in declining to listen to the arguments of the circle-squarer; but, despite great advances, and despite the assured results of Dedekind, of Cantor, of Weierstrass and of various others, the general theory of the logic of the numbers, rational and irrational, still presents several important features of great obscurity; and the philosophy of the concepts of geometry yet remains, in several very notable respects, unconquered territory, despite the work of Hilbert and of Pieri, and of our author himself. The ordinary inventors of the perpetual motion machines still stand in conflict with accepted generalizations; but nobody knows as yet what the final form of the theory of energy will be, nor can any one say precisely what place the phenomena of the radioactive bodies will occupy in that theory. The alchemists would not be welcome workers in modern laboratories; yet some sorts of transformation and of evolution of the elements are to-day

matters which theory can find it convenient, upon occasion, to treat as more or less exactly definable possibilities; while some newly observed phenomena tend to indicate, not indeed that the ancient hopes of the alchemists were well founded, but that the ultimate constitution of matter is something more fluent, less invariant, than the theoretical orthodoxy of a recent period supposed. Again, regarding the foundations of biology, a theoretical orthodoxy grows less possible, less definable, less conceivable (even as a hope) the more knowledge advances. Once 'mechanism' and 'vitalism' were mutually contradictory theories regarding the ultimate constitution of living bodies. Now they are obviously becoming more and more 'points of view,' diverse but not necessarily conflicting. So far as you find it convenient to limit your study of vital processes to those phenomena which distinguish living matter from all other natural objects, you may assume, in the modern 'pragmatic' sense, the attitude of a 'neo-vitalist.' So far, however, as you are able to lay stress, with good results, upon the many ways in which the life processes can be assimilated to those studied in physics and in chemistry, you work as if you were a partisan of 'mechanics.' In any case, your special science prospers by reason of the empirical discoveries that you make. And your theories, whatever they are, must not run counter to any positive empirical results. But otherwise, scientific orthodoxy no longer predetermines what alone it is respectable for you to think about the nature of living substance.

This gain in the freedom of theory, coming, as it does, side by side with a constant increase of a positive knowledge of nature, lends itself to various interpretations, and raises various obvious questions.

II.

One of the most natural of these interpretations, one of the most obvious of these questions, may be readily stated. Is not the lesson of all these recent discussions simply this, that general theories are simply vain, that a philosophy of nature is an idle dream, and that the results of science are coextensive with the range of actual empirical observation and of successful prediction? If this is indeed the lesson, then the decline of theoretical orthodoxy in science is—like the eclipse of dogma in religion—

merely a further lesson in pure positivism, another proof that man does best when he limits himself to thinking about what can be found in human experience, and in trying to plan what can be done to make human life more controllable and more reasonable. What we are free to do as we please—is it any longer a serious business? What we are free to think as we please—is it of any further interest to one who is in search of truth? If certain general theories are mere conceptual constructions, which to-day are, and to-morrow are cast into the oven, why dignify them by the name of philosophy? Has science any place for such theories? Why be a ‘neo-vitalist,’ or an ‘evolutionist,’ or an ‘atomist,’ or an ‘Energetiker’? Why not say, plainly: “Such and such phenomena, thus and thus described, have been observed; such and such experiences are to be expected, since the hypotheses by the terms of which we are required to expect them have been verified too often to let us regard the agreement with experience as due merely to chance; so much then with reasonable assurance we know; all else is silence—or else is some matter to be tested by another experiment?” Why not limit our philosophy of science strictly to such a counsel of resignation? Why not substitute, for the old scientific orthodoxy, simply a confession of ignorance, and a resolution to devote ourselves to the business of enlarging the bounds of actual empirical knowledge?

Such comments upon the situation just characterized are frequently made. Unfortunately, they seem not to content the very age whose revolt from the orthodoxy of traditional theory, whose uncertainty about all theoretical formulations, and whose vast wealth of empirical discoveries and of rapidly advancing special researches, would seem most to justify these very comments. Never has there been better reason than there is to-day to be content, if rational man could be content, with a pure positivism. The splendid triumphs of special research in the most various fields, the constant increase in our practical control over nature—these, our positive and growing possessions, stand in glaring contrast to the failure of the scientific orthodoxy of a former period to fix the outlines of an ultimate creed about the nature of the knowable universe. Why not ‘take the cash and let the credit go’? Why pursue the elusive theoretical ‘unification’ any further, when

what we daily get from our sciences is an increasing wealth of detailed information and of practical guidance?

As a fact, however, the known answer of our own age to these very obvious comments is a constant multiplication of new efforts towards large and unifying theories. If theoretical orthodoxy is no longer clearly definable, theoretical construction was never more rife. The history of the doctrine of evolution, even in its most recent phases, when the theoretical uncertainties regarding the 'factors of evolution' are most insisted upon, is full of illustrations of this remarkable union of scepticism in critical work with courage regarding the use of the scientific imagination. The history of those controversies regarding theoretical physics, some of whose principal phases M. Poincaré, in his book, sketches with the hand of the master, is another illustration of the consciousness of the time. Men have their freedom of thought in these regions; and they feel the need of making constant and constructive use of this freedom. And the men who most feel this need are by no means in the majority of cases professional metaphysicians—or students who, like myself, have to view all these controversies amongst the scientific theoreticians from without as learners. These large theoretical constructions are due, on the contrary, in a great many cases to special workers, who have been driven to the freedom of philosophy by the oppression of experience, and who have learned in the conflict with special problems the lesson that they now teach in the form of general ideas regarding the philosophical aspects of science.

Why, then, does science actually need general theories, despite the fact that these theories inevitably alter and pass away? What is the service of a philosophy of science, when it is certain that the philosophy of science which is best suited to the needs of one generation must be superseded by the advancing insight of the next generation? Why must that which endlessly grows, namely, man's knowledge of the phenomenal order of nature, be constantly united in men's minds with that which is certain to decay, namely, the theoretical formulation of special knowledge in more or less completely unified systems of doctrine?

I understand our author's volume to be in the main an answer to this question. To be sure, the compact and manifold teachings

which this text contains relate to a great many different special issues. A student interested in the problems of the philosophy of mathematics, or in the theory of probabilities, or in the nature and office of mathematical physics, or in still other problems belonging to the wide field here discussed, may find what he wants here and there in the text, even in case the general issues which give the volume its unity mean little to him, or even if he differs from the author's views regarding the principal issues of the book. But in the main, this volume must be regarded as what its title indicates—a critique of the nature and place of hypothesis in the work of science and a study of the logical relations of theory and fact. The result of the book is a substantial justification of the scientific utility of theoretical construction—an abandonment of dogma, but a vindication of the rights of the constructive reason.

III.

The most notable of the results of our author's investigation of the logic of scientific theories relates, as I understand his work, to a topic which the present state of logical investigation, just summarized, makes especially important, but which has thus far been very inadequately treated in the text-books of inductive logic. The useful hypotheses of science are of two kinds:.

1. The hypotheses which are valuable *precisely* because they are either verifiable or else refutable through a definite appeal to the tests furnished by experience; and

2. The hypotheses which, despite the fact that experience suggests them, are valuable *despite*, or even *because*, of the fact that experience can *neither* confirm nor refute them. The contrast between these two kinds of hypotheses is a prominent topic of our author's discussion.

Hypotheses of the general type which I have here placed first in order are the ones which the text-books of inductive logic and those summaries of scientific method which are customary in the course of the elementary treatises upon physical science are already accustomed to recognize and to characterize. The value of such hypotheses is indeed undoubted. But hypotheses of the type which I have here named in the second place are far less frequently recognized in a perfectly explicit way as useful aids in

the work of special science. One usually either fails to admit their presence in scientific work, or else remains silent as to the reasons of their usefulness. Our author's treatment of the work of science is therefore especially marked by the fact that he explicitly makes prominent both the existence and the scientific importance of hypotheses of this second type. They occupy in his discussion a place somewhat analogous to each of the two distinct positions occupied by the 'categories' and the 'forms of sensibility,' on the one hand, and by the 'regulative principles of the reason,' on the other hand, in the Kantian theory of our knowledge of nature. That is, these hypotheses which can neither be confirmed nor refuted by experience appear, in M. Poincaré's account, partly (like the conception of 'continuous quantity') as devices of the understanding whereby we give conceptual unity and an invisible connectedness to certain types of phenomenal facts which come to us in a discrete form and in a confused variety; and partly (like the larger organizing concepts of science) as principles regarding the structure of the world in its wholeness; *i. e.*, as principles in the light of which we try to interpret our experience, so as to give to it a totality and an inclusive unity such as Euclidean space, or such as the world of the theory of energy is conceived to possess. Thus viewed, M. Poincaré's logical theory of this second class of hypotheses undertakes to accomplish, with modern means and in the light of to-day's issues, a part of what Kant endeavored to accomplish in his theory of scientific knowledge with the limited means which were at his disposal. Those aspects of science which are determined by the use of the hypotheses of this second kind appear in our author's account as constituting an essential human way of viewing nature, an interpretation rather than a portrayal or a prediction of the objective facts of nature, an adjustment of our conceptions of things to the internal needs of our intelligence, rather than a grasping of things as they are in themselves.

To be sure, M. Poincaré's view, in this portion of his work, obviously differs, meanwhile, from that of Kant, as well as this agrees, in a measure, with the spirit of the Kantian epistemology. I do not mean therefore to class our author as a Kantian. For Kant, the interpretations imposed by the 'forms of sensibility,'

and by the 'categories of the understanding,' upon our doctrine of nature are rigidly predetermined by the unalterable 'form' of our intellectual powers. We 'must' thus view facts, whatever the data of sense must be. This, of course, is not M. Poincaré's view. A similarly rigid predetermination also limits the Kantian 'ideas of the reason' to a certain set of principles whose guidance of the course of our theoretical investigations is indeed only 'regulative,' but is 'a priori,' and so unchangeable. For M. Poincaré, on the contrary, all this adjustment of our interpretations of experience to the needs of our intellect is something far less rigid and unalterable, and is constantly subject to the suggestions of experience. We must indeed interpret in our own way; but our way is itself only relatively determinate; it is essentially more or less plastic; other interpretations of experience are conceivable. Those that we use are merely the ones found to be most convenient. But this convenience is not absolute necessity. Unverifiable and irrefutable hypotheses in science are indeed, in general, indispensable aids to the organization and to the guidance of our interpretation of experience. But it is experience itself which points out to us what lines of interpretation will prove most convenient. Instead of Kant's rigid list of *a priori* 'forms,' we consequently have in M. Poincaré's account a set of conventions, neither wholly subjective and arbitrary, nor yet imposed upon us unambiguously by the external compulsion of experience. The organization of science, so far as this organization is due to hypotheses of the kind here in question, thus resembles that of a constitutional government—neither absolutely necessary, nor yet determined apart from the will of the subjects, nor yet accidental—a free, yet not a capricious establishment of good order, in conformity with empirical needs.

Characteristic remains, however, for our author, as, in his decidedly contrasting way, for Kant, the thought that *without principles which at every stage transcend precise confirmation through such experience as is then accessible the organization of experience is impossible*. Whether one views these principles as conventions or as *a priori* 'forms,' they may therefore be described as hypotheses, but as hypotheses that, while lying at the basis of our actual physical sciences, at once refer to experience and help

us in dealing with experience, and are yet neither confirmed nor refuted by the experiences which we possess or which we can hope to attain.

Three special instances or classes of instances, according to our author's account, may be used as illustrations of this general type of hypotheses. They are: (1) The hypothesis of the existence of continuous extensive *quanta* in nature; (2) The principles of geometry; (3) The principles of mechanics and of the general theory of energy. In case of each of these special types of hypotheses we are at first disposed, apart from reflection, to say that we *find* the world to be thus or thus, so that, for instance, we can confirm the thesis according to which nature contains continuous magnitudes; or can prove or disprove the physical truth of the postulates of Euclidean geometry; or can confirm by definite experience the objective validity of the principles of mechanics. A closer examination reveals, according to our author, the incorrectness of all such opinions. Hypotheses of these various special types are needed; and their usefulness can be empirically shown. They are in touch with experience; and that they are not merely arbitrary conventions is also verifiable. They are not *a priori* necessities; and we can easily conceive intelligent beings whose experience could be best interpreted without using these hypotheses. Yet these hypotheses are *not* subject to direct confirmation or refutation by experience. They stand then in sharp contrast to the scientific hypotheses of the other, and more frequently recognized, type, *i. e.*, to the hypotheses which *can* be tested by a definite appeal to experience. To these other hypotheses our author attaches, of course, great importance. His treatment of them is full of a living appreciation of the significance of empirical investigation. But the central problem of the logic of science thus becomes the problem of the relation between the two fundamentally distinct types of hypotheses, *i. e.*, between those which can not be verified or refuted through experience, and those which can be empirically tested.

IV.

The detailed treatment which M. Poincaré gives to the problem thus defined must be learned from his text. It is no part of my purpose to expound, to defend or to traverse any of his special

conclusions regarding this matter. Yet I can not avoid observing that, while M. Poincaré strictly confines his illustrations and his expressions of opinion to those regions of science wherein, as special investigator, he is himself most at home, the issues which he thus raises regarding the logic of science are of even more critical importance and of more impressive interest when one applies M. Poincaré's methods to the study of the concepts and presuppositions of the organic and of the historical and social sciences, than when one confines one's attention, as our author here does, to the physical sciences. It belongs to the province of an introduction like the present to point out, however briefly and inadequately, that the significance of our author's ideas extends far beyond the scope to which he chooses to confine their discussion.

The historical sciences, and in fact all those sciences such as geology, and such as the evolutionary sciences in general, undertake theoretical constructions which relate to past time. Hypotheses relating to the more or less remote past stand, however, in a position which is very interesting from the point of view of the logic of science. Directly speaking, no such hypothesis is capable of confirmation or of refutation, because we can not return into the past to verify by our own experience what then happened. Yet indirectly, such hypotheses may lead to predictions of coming experience. These latter will be subject to control. Thus, Schliemann's confidence that the legend of Troy had a definite historical foundation led to predictions regarding what certain excavations would reveal. In a sense somewhat different from that which filled Schliemann's enthusiastic mind, these predictions proved verifiable. The result has been a considerable change in the attitude of historians toward the legend of Troy. Geological investigation leads to predictions regarding the order of the strata or the course of mineral veins in a district, regarding the fossils which may be discovered in given formations, and so on. These hypotheses are subject to the control of experience. The various theories of evolutionary doctrine include many hypotheses capable of confirmation and of refutation by empirical tests. Yet, despite all such empirical control, it still remains true that whenever a science is mainly concerned with the remote past, whether this science be archeology, or geology, or anthro-

pology, or Old Testament history, the principal theoretical constructions always include features which no appeal to present or to accessible future experience can ever definitely test. Hence the suspicion with which students of experimental science often regard the theoretical constructions of their confrères of the sciences that deal with the past. The origin of the races of men, of man himself, of life, of species, of the planet; the hypotheses of anthropologists, of archeologists, of students of 'higher criticism'—all these are matters which the men of the laboratory often regard with a general incredulity as belonging not at all to the domain of true science. Yet no one can doubt the importance and the inevitableness of endeavoring to apply scientific method to these regions also. Science needs theories regarding the past history of the world. And no one who looks closer into the methods of these sciences of past time can doubt that verifiable and unverifiable hypotheses are in all these regions inevitably interwoven; so that, while experience is always the guide, the attitude of the investigator towards experience is determined by interests which have to be partially due to what I should call that 'internal meaning,' that human interest in rational theoretical construction which inspires the scientific inquiry; and the theoretical constructions which prevail in such sciences are neither unbiased reports of the actual constitution of an external reality, nor yet arbitrary constructions of fancy. These constructions in fact resemble in a measure those which M. Poincaré in this book has analyzed in the case of geometry. They are constructions molded, but *not* predetermined in their details, by experience. We report facts; we let the facts speak; but we, as we investigate, in the popular phrase, 'talk back' to the facts. We interpret as well as report. Man is not merely made for science, but science is made for man. It expresses his deepest intellectual needs, as well as his careful observations. It is an effort to bring internal meanings into harmony with external verifications. It attempts therefore to control, as well as to submit, to conceive with rational unity, as well as to accept data. Its arts are those directed towards self-possession as well as towards an imitation of the outer reality which we find. It seeks therefore a disciplined freedom of thought. The discipline is as essential as the freedom; but

the latter has also its place. The theories of science are human, as well as objective, internally rational, as well as (when that is possible) subject to external tests.

In a field very different from that of the historical sciences, namely, in a science of observation and of experiment, which is at the same time an organic science, I have been led in the course of some study of the history of certain researches to notice the existence of a theoretical conception which has proved extremely fruitful in guiding research, but which apparently resembles in a measure the type of hypotheses of which M. Poincaré speaks when he characterizes the principles of mechanics and of the theory of energy. I venture to call attention here to this conception, which seems to me to illustrate M. Poincaré's view of the functions of hypothesis in scientific work.

The modern science of pathology is usually regarded as dating from the earlier researches of Virchow, whose 'Cellular Pathology' was the outcome of a very careful and elaborate induction. Virchow, himself, felt a strong aversion to mere speculation. He endeavored to keep close to observation, and to relieve medical science from the control of fantastic theories, such as those of the *Naturphilosophen* had been. Yet Virchow's researches were, as early as 1847, or still earlier, already under the guidance of a theoretical presupposition which he himself states as follows: "We have learned to recognize," he says, "that diseases are not autonomous organisms, that they are no entities that have entered into the body, that they are no parasites which take root in the body, but that *they merely show us the course of the vital processes under altered conditions*" ('dass sie nur Ablauf der Lebenserscheinungen unter veränderten Bedingungen darstellen').

The enormous importance of this theoretical presupposition for all the early successes of modern pathological investigation is generally recognized by the experts. I do not doubt this opinion. It appears to be a commonplace of the history of this science. But in Virchow's later years, this very presupposition seemed to some of his contemporaries to be called in question by the successes of recent bacteriology. The question arose whether the theoretical foundations of Virchow's pathology had not been set aside. And in fact, the theory of the parasitical origin

of a vast number of diseased conditions has indeed come upon an empirical basis to be generally recognized. Yet to the end of his own career, Virchow stoutly maintained that in all its essential significance his own fundamental principle remained quite untouched by the newer discoveries. And, as a fact, this view could indeed be maintained. For if diseases proved to be the consequences of the presence of parasites, the diseases themselves, so far as they belonged to the diseased organism, were still not the parasites, but were, as before, the reaction of the organism to the *veränderte Bedingungen* which the presence of the parasites entailed. So Virchow could well insist. And if the famous principle in question is only stated with sufficient generality, it amounts simply to saying that if a disease involves a change in an organism, and if this change is subject to law at all, then the nature of the organism and the reaction of the organism to whatever it is which causes the disease must be understood in case the disease is to be understood.

For this very reason, however, Virchow's theoretical principle in its most general form *could be neither confirmed nor refuted by experience*. It would remain empirically irrefutable, so far as I can see, even if we should learn that the devil was the true cause of all diseases. For the devil himself would then simply predetermine the *veränderte Bedingungen* to which the diseased organism would be reacting. Let bullets or bacteria, poisons or compressed air, or the devil be the *Bedingungen* to which a diseased organism reacts, the postulate that Virchow states in the passage just quoted will remain irrefutable, if only this postulate be interpreted to meet the case. For the principle in question merely says that whatever entity it may be, bullet, or poison, or devil, that affects the organism, the disease is not that entity, but is the resulting alteration in the process of the organism.

I insist, then, that this principle of Virchow's is no trial supposition, no scientific hypothesis in the narrower sense—capable of being submitted to precise empirical tests. It is, on the contrary, a very precious *leading idea*, a theoretical interpretation of phenomena, in the light of which observations are to be made—'a regulative principle' of research. It is equivalent to a resolution to search for those detailed connections which link

the processes of disease to the normal process of the organism. Such a search undertakes to find the true unity, whatever that may prove to be, wherein the pathological and the normal processes are linked. Now without some such leading idea, the cellular pathology itself could never have been reached; because the empirical facts in question would never have been observed. Hence this principle of Virchow's was indispensable to the growth of his science. Yet it was not a verifiable and not a refutable hypothesis. One value of unverifiable and irrefutable hypotheses of this type lies, then, in the sort of empirical inquiries which they initiate, inspire, organize and guide. In these inquiries hypotheses in the narrower sense, that is, trial propositions which are to be submitted to definite empirical control, are indeed everywhere present. And the use of the other sort of principles lies wholly in their application to experience. Yet without what I have just proposed to call the 'leading ideas' of a science, that is, its principles of an unverifiable and irrefutable character, suggested, but not to be finally tested, by experience, the hypotheses in the narrower sense would lack that guidance which, as M. Poincaré has shown, the larger ideas of science give to empirical investigation.

V.

I have dwelt, no doubt, at too great length upon one aspect only of our author's varied and well-balanced discussion of the problems and concepts of scientific theory. Of the hypotheses in the narrower sense and of the value of direct empirical control, he has also spoken with the authority and the originality which belong to his position. And in dealing with the foundations of mathematics, he has raised one or two questions of great philosophical import into which I have no time, even if I had the right, to enter here. In particular, in speaking of the essence of mathematical reasoning, and of the difficult problem of what makes possible novel results in the field of pure mathematics, M. Poincaré defends a thesis regarding the office of 'demonstration by recurrence'—a thesis which is indeed disputable, which has been disputed and which I myself should be disposed, so far as I at present understand the matter, to modify in some respects, even in accepting the spirit of our author's assertion. Yet there

can be no doubt of the importance of this thesis, and of the fact that it defines a characteristic that is indeed fundamental in a wide range of mathematical research. The philosophical problems that lie at the basis of recurrent proofs and processes are, as I have elsewhere argued, of the most fundamental importance.

These, then, are a few hints relating to the significance of our author's discussion, and a few reasons for hoping that our own students will profit by the reading of the book as those of other nations have already done.

Of the person and of the life-work of our author a few words are here, in conclusion, still in place, addressed, not, to the students of his own science, to whom his position is well known, but to the general reader who may seek guidance in these pages.

Jules Henri Poincaré was born at Nancy, in 1854, the son of a professor in the Faculty of Medicine at Nancy. He studied at the École Polytechnique and at the École des Mines, became first an engineer, and later received his doctorate in mathematics in 1879. In 1883 he began courses of instruction in mathematics at the École Polytechnique; in 1886 received a professorship of mathematical physics in the Faculty of Sciences at Paris; then became member of the Academy of Sciences at Paris, in 1887, and has devoted his life to instruction and investigation in the regions of pure mathematics, of mathematical physics and of celestial mechanics. His list of published treatises relating to various branches of his chosen sciences is long; and his original memoirs have included several momentous investigations, which have gone far to transform more than one branch of research. His presence recently at the International Congress of Arts and Science in St. Louis was one of the most noticeable features of that remarkable gathering of distinguished foreign guests. In Poincaré the reader meets, then, not one who is primarily a speculative student of general problems for their own sake, but an original investigator of the highest rank in several distinct, although interrelated, branches of modern research. The theory of functions—a highly recondite region of pure mathematics—owes to him advances of the first importance, for instance, the definition of a new type of functions. The 'problem of the three bodies,' a famous and fundamental problem of celestial mechanics,

has received from his studies a treatment whose significance has been recognized by the highest authorities. His international reputation has been confirmed by the conferring of more than one important prize for his researches. His membership in the most eminent learned societies of various nations is widely extended; his volumes bearing upon various branches of mathematics and of mathematical physics are used by special students in all parts of the learned world; in brief, he is, as geometer, as analyst and as a theoretical physicist, a leader of his age.

Meanwhile, as contributor to the philosophical discussion of the bases and methods of science, M. Poincaré has long been active. When, in 1893, the admirable *Revue de Métaphysique et de Morale* began to appear, M. Poincaré was soon found amongst the most satisfactory of the contributors to the work of that journal, whose office it has especially been to bring philosophy and the various special sciences (both natural and moral) into a closer mutual understanding. The discussions brought together in the present volume are in large part the outcome of M. Poincaré's contributions to the *Revue de Métaphysique et de Morale*. The reader of M. Poincaré's book is in presence, then, of a great special investigator who is also a philosopher.

SCIENCE AND HYPOTHESIS.

INTRODUCTION.

For a superficial observer, scientific truth is beyond the possibility of doubt; the logic of science is infallible, and if the scientists are sometimes mistaken, this is only from their mistaking its rules.

“The mathematical verities flow from a small number of self-evident propositions by a chain of impeccable reasonings; they impose themselves not only on us, but on nature itself. They fetter, so to speak, the Creator and only permit him to choose between some relatively few solutions. A few experiments then will suffice to let us know what choice he has made. From each experiment a crowd of consequences will follow by a series of mathematical deductions, and thus each experiment will make known to us a corner of the universe.”

Behold what is for many people in the world, for scholars getting their first notions of physics, the origin of scientific certitude. This is what they suppose to be the rôle of experimentation and mathematics. This same conception, a hundred years ago, was held by many savants who dreamed of constructing the world with as little as possible taken from experiment.

On a little more reflection it was perceived how great a place hypothesis occupies; that the mathematician can not do without it, still less the experimenter. And then it was doubted if all these constructions were really solid, and believed that a breath would overthrow them. To be skeptical in this fashion is still to be superficial. To doubt everything and to believe everything are two equally convenient solutions; each saves us from thinking.

Instead of pronouncing a summary condemnation, we ought therefore to examine with care the rôle of hypothesis; we shall then recognize, not only that it is necessary, but that usually it is legitimate. We shall also see that there are several sorts of hy-

potheses; that some are verifiable, and once confirmed by experiment become fruitful truths; that others, powerless to lead us astray, may be useful to us in fixing our ideas; that others, finally, are hypotheses only in appearance and are reducible to disguised definitions or conventions.

These last are met with above all in mathematics and the related sciences. Thence precisely it is that these sciences get their rigor; these conventions are the work of the free activity of our mind, which, in this domain, recognizes no obstacle. Here our mind can affirm, since it decrees; but let us understand that while these decrees are imposed upon *our* science, which, without them, would be impossible, they are not imposed upon nature. Are they then arbitrary? No, else were they sterile. Experiment leaves us our freedom of choice, but it guides us by aiding us to discern the easiest way. Our decrees are therefore like those of a prince, absolute but wise, who consults his Council of State.

Some people have been struck by this character of free convention recognizable in certain fundamental principles of the sciences. They have wished to generalize beyond measure, and, at the same time, they have forgotten that liberty is not license. Thus they have reached what is called *nominalism*, and have asked themselves if the savant is not the dupe of his own definitions and if the world he thinks he discovers is not simply created by his own caprice.* Under these conditions science would be certain, but deprived of significance.

If this were so, science would be powerless. Now every day we see it work under our very eyes. That could not be if it taught us nothing of reality. Still, the things themselves are not what it can reach, as the naïve dogmatists think, but only the relations between things. Outside of these relations there is no knowable reality.

Such is the conclusion to which we shall come, but for that we must review the series of sciences from arithmetic and geometry to mechanics and experimental physics.

What is the nature of mathematical reasoning? Is it really deductive, as is commonly supposed? A deeper analysis shows us

* See Le Roy, *Science et Philosophie. Revue de Métaphysique et de Morale*, 1901.

that it is not, that it partakes in a certain measure of the nature of inductive reasoning, and just because of this is it so fruitful. None the less does it retain its character of rigor absolute; this is the first thing that had to be shown.

Knowing better now one of the instruments which mathematics puts into the hands of the investigator, we had to analyze another fundamental notion, that of mathematical magnitude. Do we find it in nature, or do we ourselves introduce it there? And, in this latter case, do we not risk marring everything? Comparing the rough data of our senses with that extremely complex and subtle concept which mathematicians call magnitude, we are forced to recognize a difference; this frame into which we wish to force everything is of our own construction; but we have not made it at random. We have made it, so to speak, by measure and therefore we can make the facts fit into it without changing what is essential in them.

Another frame which we impose on the world is space. Whence come the first principles of geometry? Are they imposed on us by logic? Lobachevski has proved not, by creating non-Euclidean geometry. Is space revealed to us by our senses? Still no, for the space our senses could show us differs absolutely from that of the geometer. Is experience the source of geometry? A deeper discussion will show us it is not. We therefore conclude that the first principles of geometry are only conventions; but these conventions are not arbitrary and if transported into another world (that I call the non-Euclidean world and seek to imagine), then we should have been led to adopt others.

In mechanics we should be led to analogous conclusions, and should see that the principles of this science, though more directly based on experiment, still partake of the conventional character of the geometric postulates. Thus far nominalism triumphs; but now we arrive at the physical sciences, properly so called. Here the scene changes; we meet another sort of hypotheses and we see their fertility. Without doubt, at first blush, the theories seem to us fragile, and the history of science proves to us how ephemeral they are; yet they do not entirely perish, and of each of them something remains. It is this something we must seek to disentangle, since there and there alone is the veritable reality.

The method of the physical sciences rests on the induction which makes us expect the repetition of a phenomenon when the circumstances under which it first happened are reproduced. If *all* these circumstances could be reproduced at once, this principle could be applied without fear; but that will never happen; some of these circumstances will always be lacking. Are we absolutely sure they are unimportant? Evidently not. That may be probable, it can not be rigorously certain. Hence the important rôle the notion of probability plays in the physical sciences. The calculus of probabilities is therefore not merely a recreation or a guide to players of baccarat, and we must seek to go deeper with its foundations. Under this head I have been able to give only very incomplete results, so strongly does this vague instinct which lets us discern probability defy analysis.

After a study of the conditions under which the physicist works, I have thought proper to show him at work. For that I have taken instances from the history of optics and of electricity. We shall see whence have sprung the ideas of Fresnel, of Maxwell, and what unconscious hypotheses were made by Ampère and the other founders of electrodynamics.

PART I.

NUMBER AND MAGNITUDE.

CHAPTER I.

ON THE NATURE OF MATHEMATICAL REASONING.

I.

THE very possibility of the science of mathematics seems an insoluble contradiction. If this science is deductive only in appearance, whence does it derive that perfect rigor no one dreams of doubting? If, on the contrary, all the propositions it enunciates can be deduced one from another by the rules of formal logic, why is not mathematics reduced to an immense tautology? The syllogism can teach us nothing essentially new, and, if everything is to spring from the principle of identity, everything should be capable of being reduced to it. Shall we then admit that the enunciations of all those theorems which fill so many volumes are nothing but devious ways of saying A is A !

Without doubt, we can go back to the axioms, which are at the source of all these reasonings. If we decide that these can not be reduced to the principle of contradiction, if still less we see in them experimental facts which could not partake of mathematical necessity, we have yet the resource of classing them among synthetic *a priori* judgments. This is not to solve the difficulty, but only to baptize it; and even if the nature of synthetic judgments were for us no mystery, the contradiction would not have disappeared, it would only have moved back; syllogistic reasoning remains incapable of adding anything to the data given it; these data reduce themselves to a few axioms, and we should find nothing else in the conclusions.

No theorem could be new if no new axiom intervened in its demonstration; reasoning could give us only the immediately evident verities borrowed from direct intuition; it would be only

an intermediary parasite, and therefore should we not have good reason to ask whether the whole syllogistic apparatus did not serve solely to disguise our borrowing?

The contradiction will strike us the more if we open any book on mathematics; on every page the author will announce his intention of generalizing some proposition already known. Does the mathematical method proceed from the particular to the general, and, if so, how then can it be called deductive?

If finally the science of number were purely analytic, or could be analytically derived from a small number of synthetic judgments, it seems that a mind sufficiently powerful could at a glance perceive all its truths; nay more, we might even hope that some day one would invent to express them a language sufficiently simple to have them appear self-evident to an ordinary intelligence.

If we refuse to admit these consequences, it must be conceded that mathematical reasoning has of itself a sort of creative virtue and consequently differs from the syllogism.

The difference must even be profound. We shall not, for example, find the key to the mystery in the frequent use of that rule according to which one and the same uniform operation applied to two equal numbers will give identical results.

All these modes of reasoning, whether or not they be reducible to the syllogism properly so called, retain the analytic character, and just because of that are powerless.

II.

The discussion is old; Leibnitz tried to prove 2 and 2 make 4; let us look a moment at his demonstration.

I will suppose the number 1 defined and also the operation $x + 1$ which consists in adding unity to a given number x .

These definitions, whatever they be, do not enter into the course of the reasoning.

I define then the numbers 2, 3 and 4 by the equalities

$$(1) \quad 1 + 1 = 2; \quad (2) \quad 2 + 1 = 3; \quad (3) \quad 3 + 1 = 4.$$

In the same way, I define the operation $x + 2$ by the relation:

$$(4) \quad x + 2 = (x + 1) + 1.$$

That presupposed, we have

$$2 + 2 = (2 + 1) + 1 \quad (\text{Definition 4}),$$

$$2 + 1 + 1 = 3 + 1 \quad (\text{Definition 2}),$$

$$3 + 1 = 4 \quad (\text{Definition 3}),$$

whence

$$2 + 2 = 4 \quad \text{Q. E. D.}$$

It can not be denied that this reasoning is purely analytic. But ask any mathematician: 'That is not a demonstration properly so called,' he will say to you: 'that is a verification.' We have confined ourselves to comparing two purely conventional definitions and have ascertained their identity; we have learned nothing new. *Verification* differs from true demonstration precisely because it is purely analytic and because it is sterile. It is sterile because the conclusion is nothing but the premises translated into another language. On the contrary, true demonstration is fruitful because the conclusion here is in a sense more general than the premises.

The equality $2 + 2 = 4$ is thus susceptible of a verification only because it is particular. Every particular enunciation in mathematics can always be verified in this same way. But if mathematics could be reduced to a series of such verifications, it would not be a science. So a chess-player, for example, does not create a science in winning a game. There is no science apart from the general.

It may even be said the very object of the exact sciences is to spare us these direct verifications.

III.

Let us therefore see the geometer at work and seek to catch his process.

The task is not without difficulty; it does not suffice to open a work at random and analyze any demonstration in it.

We must first exclude geometry, where the question is complicated by arduous problems relative to the rôle of the postulates, to the nature and the origin of the notion of space. For analogous reasons we can not turn to the infinitesimal analysis. We must seek mathematical thought where it has remained pure, that is, in arithmetic.

A choice still is necessary; in the higher parts of the theory of numbers, the primitive mathematical notions have already undergone an elaboration so profound that it becomes difficult to analyze them.

It is, therefore, at the beginning of arithmetic that we must expect to find the explanation we seek, but it happens that precisely in the demonstration of the most elementary theorems the authors of the classic treatises have shown the least precision and rigor. We must not impute this to them as a crime; they have yielded to a necessity; beginners are not prepared for real mathematical rigor; they would see in it only useless and irksome subtleties; it would be a waste of time to try prematurely to make them more exacting; they must pass over rapidly, but without skipping stations, the road traversed slowly by the founders of the science.

Why is so long a preparation necessary to become habituated to this perfect rigor, which, it would seem, should naturally impress itself upon all good minds? This is a logical and psychological problem well worthy of study.

But we shall not take it up; it is foreign to our purpose; all I wish to insist on is that, not to fail of our purpose, we must recast the demonstrations of the most elementary theorems and give them, not the crude form in which they are left, so as not to harass beginners, but the form that will satisfy a skilled geometer.

DEFINITION OF ADDITION.—I suppose already defined the operation $x + 1$, which consists in adding the number 1 to a given number x .

This definition, whatever it be, does not enter into our subsequent reasoning.

We now have to define the operation $x + a$, which consists in adding the number a to a given number x .

Supposing we have defined the operation

$$x + (a - 1),$$

the operation $x + a$ will be defined by the equality

$$(1) \quad x + a = [x + (a - 1)] + 1.$$

We shall know then what $x + a$ is when we know what $x + (a - 1)$ is, and as I have supposed that to start with we

knew what $x + 1$ is, we can define successively and 'by recurrence' the operations $x + 2$, $x + 3$, etc.

This definition deserves a moment's attention; it is of a particular nature which already distinguishes it from the purely logical definition; the equality (1) contains an infinity of distinct definitions, each having a meaning only when one knows the preceding.

PROPERTIES OF ADDITION.—*Associativity*.—I say that

$$a + (b + c) = (a + b) + c.$$

In fact the theorem is true for $c = 1$; it is then written

$$a + (b + 1) = (a + b) + 1,$$

which, apart from the difference of notation, is nothing but the equality (1), by which I have just defined addition.

Supposing the theorem true for $c = \gamma$, I say it will be true for $c = \gamma + 1$.

In fact, supposing

$$(a + b) + \gamma = a + (b + \gamma),$$

it follows that

$$[(a + b) + \gamma] + 1 = [a + (b + \gamma)] + 1$$

or by definition (1)

$$(a + b) + (\gamma + 1) = a + (b + \gamma + 1) = a + [b + (\gamma + 1)],$$

which shows, by a series of purely analytic deductions, that the theorem is true for $\gamma + 1$.

Being true for $c = 1$, we thus see successively that so it is for $c = 2$, for $c = 3$, etc.

Commutativity.—1° I say that

$$a + 1 = 1 + a.$$

The theorem is evidently true for $a = 1$; we can *verify* by purely analytic reasoning that if it is true for $a = \gamma$ it will be true for $a = \gamma + 1$; for then

$$(\gamma + 1) + 1 = (1 + \gamma) + 1 = 1 + (\gamma + 1);$$

now it is true for $a = 1$, therefore it will be true for $a = 2$, for $a = 3$, etc., which is expressed by saying that the enunciated proposition is demonstrated by recurrence.

2° I say that

$$a + b = b + a.$$

The theorem has just been demonstrated for $b = 1$; it can be *verified* analytically that if it is true for $b = \beta$, it will be true for $b = \beta + 1$.

The proposition is therefore established by recurrence.

DEFINITION OF MULTIPLICATION.—We shall define multiplication by the equalities

$$(1) \quad a \times 1 = a.$$

$$(2) \quad a \times b = [a \times (b - 1)] + a.$$

Like equality (1), equality (2) contains an infinity of definitions; having defined $a \times 1$, it enables us to define successively: $a \times 2$, $a \times 3$, etc.

PROPERTIES OF MULTIPLICATION.—*Distributivity*.—I say that

$$(a + b) \times c = (a \times c) + (b \times c).$$

We verify analytically that the equality is true for $c = 1$; then that if the theorem is true for $c = \gamma$, it will be true for $c = \gamma + 1$.

The proposition is, therefore, demonstrated by recurrence.

Commutativity.—1° I say that

$$a \times 1 = 1 \times a.$$

The theorem is evident for $a = 1$.

We verify analytically that if it is true for $a = a$, it will be true for $a = a + 1$.

2° I say that

$$a \times b = b \times a.$$

The theorem has just been proven for $b = 1$. We could verify analytically that if it is true for $b = \beta$, it will be true for $b = \beta + 1$.

IV.

Here I stop this monotonous series of reasonings. But this very monotony has the better brought out the procedure which is uniform and is met again at each step.

This procedure is the demonstration by recurrence. We first establish a theorem for $n = 1$; then we show that if it is true of $n - 1$, it is true of n , and thence conclude that it is true for all the whole numbers.

We have just seen how it may be used to demonstrate the rules of addition and multiplication, that is to say, the rules of the alge-

braic calculus; this calculus is an instrument of transformation, which lends itself to many more differing combinations than does the simple syllogism; but it is still an instrument, purely analytic, and incapable of teaching us anything new. If mathematics had no other instrument, it would therefore be forthwith arrested in its development; but it has recourse anew to the same procedure, that is, to reasoning by recurrence, and it is able to continue its forward march.

If we look closely, at every step we meet again this mode of reasoning, either in the simple form we have just given it, or under a form more or less modified.

Here then we have the mathematical reasoning *par excellence*, and we must examine it more closely.

V.

The essential characteristic of reasoning by recurrence is that it contains, condensed, so to speak, in a single formula, an infinity of syllogisms.

That this may the better be seen, I will state one after another these syllogisms which are, if you will allow me the expression, arranged 'in cascade.'

These are of course hypothetical syllogisms.

The theorem is true of the number 1.

Now, if it is true of 1, it is true of 2.

Therefore it is true of 2.

Now, if it is true of 2, it is true of 3.

Therefore it is true of 3, and so on.

We see that the conclusion of each syllogism serves as minor to the following.

Futhermore the majors of all our syllogisms can be reduced to a single formula.

If the theorem is true of $n - 1$, so it is of n .

We see, then, that in reasoning by recurrence, we confine ourselves to stating the minor of the first syllogism, and the general formula which contains as particular cases all the majors.

This never-ending series of syllogisms is thus reduced to a phrase of a few lines.

It is now easy to comprehend why every particular consequence of a theorem can, as I have explained above, be verified by purely analytic procedures.

If instead of showing that our theorem is true of all numbers, we only wish to show it true of the number 6, for example, it will suffice for us to establish the first 5 syllogisms of our cascade; 9 would be necessary if we wished to prove the theorem for the number 10; more would be needed for a larger number; but, however great this number might be, we should always end by reaching it, and the analytic verification would be possible.

And yet, however far we thus might go, we could never rise to the general theorem, applicable to all numbers, which alone can be the object of science. To reach this, an infinity of syllogisms would be necessary; it would be necessary to overleap an abyss that the patience of the analyst, restricted to the resources of formal logic alone, never could fill up.

I asked at the outset why one could not conceive of a mind sufficiently powerful to perceive at a glance the whole body of mathematical truths.

The answer is now easy; a chess-player is able to combine four moves, five moves, in advance, but, however extraordinary he may be, he will never prepare more than a finite number of them; if he applies his faculties to arithmetic, he will not be able to perceive its general truths by a single direct intuition; to arrive at the smallest theorem he can not dispense with the aid of reasoning by recurrence, for this is an instrument which enables us to pass from the finite to the infinite.

This instrument is always useful, for, allowing us to overleap at a bound as many stages as we wish, it spares us verifications, long, irksome and monotonous, which would quickly become impracticable. But it becomes indispensable as soon as we aim at the general theorem, to which analytic verification would bring us continually nearer without ever enabling us to reach it.

In this domain of arithmetic, we may think ourselves very far from the infinitesimal analysis, and yet, as we have just seen, the idea of the mathematical infinite already plays a preponderant rôle, and without it there would be no science, because there would be nothing general.

VI.

The judgment on which reasoning by recurrence rests can be put under other forms; we may say, for example, that in an infinite collection of different whole numbers there is always one which is less than all the others.

We can easily pass from one enunciation to the other and thus get the illusion of having demonstrated the legitimacy of reasoning by recurrence. But we shall always be arrested, we shall always arrive at an undemonstrable axiom which will be in reality only the proposition to be proved translated into another language.

We can not therefore escape the conclusion that the rule of reasoning by recurrence is irreducible to the principle of contradiction.

Neither can this rule come to us from experience; experience could teach us that the rule is true for the first ten or hundred numbers, for example, it can not attain to the indefinite series of numbers, but only to a portion of this series, more or less long but always limited.

Now if it were only a question of that, the principle of contradiction would suffice; it would always allow of our developing as many syllogisms as we wished; it is only when it is a question of including an infinity of them in a single formula, it is only before the infinite that this principle fails, and there too, experience becomes powerless. This rule, inaccessible to analytic demonstration and to experience, is the veritable type of the synthetic *a priori* judgment. On the other hand, we can not think of seeing in it a convention, as in some of the postulates of geometry.

Why then does this judgment force itself upon us with an irresistible evidence? It is because it is only the affirmation of the power of the mind which knows itself capable of conceiving the indefinite repetition of the same act when once this act is possible. The mind has a direct intuition of this power, and experience can only give occasion for using it and thereby becoming conscious of it.

But, one will say, if raw experience can not legitimize reasoning by recurrence, is it so of experiment aided by induction? We see successively that a theorem is true of the number 1, of the number 2, of the number 3 and so on; the law is evident, we

say, and it has the same warranty as every physical law based on observations, whose number is very great but limited.

Here is, it must be admitted, a striking analogy with the usual procedures of induction. But there is an essential difference. Induction applied to the physical sciences is always uncertain, because it rests on the belief in a general order of the universe, an order outside of us. Mathematical induction, that is, demonstration by recurrence, on the contrary, imposes itself necessarily, because it is only the affirmation of a property of the mind itself.

VII.

Mathematicians, as I have said before, always endeavor to *generalize* the propositions they have obtained, and, to seek no other example, we have just proved the equality:

$$a + 1 = 1 + a$$

and afterwards used it to establish the equality

$$a + b = b + a$$

which is manifestly more general.

Mathematics can, therefore, like the other sciences, proceed from the particular to the general.

This is a fact which would have appeared incomprehensible to us at the outset of this study, but which is no longer mysterious to us, since we have ascertained the analogies between demonstration by recurrence and ordinary induction.

Without doubt recurrent reasoning in mathematics and inductive reasoning in physics rest on different foundations, but their march is parallel, they advance in the same sense, that is to say, from the particular to the general.

Let us examine the case a little more closely.

To demonstrate the equality

$$a + 2 = 2 + a$$

it suffices to twice apply the rule

$$(1) \quad a + 1 = 1 + a$$

and write

$$(2) \quad a + 2 = a + 1 + 1 = 1 + a + 1 = 1 + 1 + a = 2 + a.$$

The equality (2) thus deduced in purely analytic way from the equality (1) is, however, not simply a particular case of it; it is something quite different.

We can not therefore even say that in the really analytic and deductive part of mathematical reasoning, we proceed from the general to the particular in the ordinary sense of the word.

The two members of the equality (2) are simply combinations more complicated than the two members of the equality (1), and analysis only serves to separate the elements which enter into these combinations and to study their relations.

Mathematicians proceed therefore 'by construction,' they 'construct' combinations more and more complicated. Coming back then by the analysis of these combinations, of these aggregates, so to speak, to their primitive elements, they perceive the relations of these elements and from them deduce the relations of the aggregates themselves.

This is a purely analytical proceeding, but it is not, however, a proceeding from the general to the particular, because evidently the aggregates can not be regarded as more particular than their elements.

Great importance, and justly, has been attached to this procedure of 'construction,' and some have tried to see in it the necessary and sufficient condition for the progress of the exact sciences.

Necessary, without doubt; but sufficient, no.

For a construction to be useful and not a vain toil for the mind, that it may serve as stepping-stone to one wishing to mount, it must first of all possess a sort of unity enabling us to see in it something besides the juxtaposition of its elements.

Or, more exactly, there must be some advantage in considering the construction rather than its elements themselves.

What can this advantage be?

Why reason on a polygon, for instance, which is always decomposable into triangles, and not on the elementary triangles?

It is because there are properties appertaining to polygons of any number of sides and that may be immediately applied to any particular polygon.

Usually, on the contrary, it is only at the cost of the most

prolonged exertions that they could be found by studying directly the relations of the elementary triangles. The knowledge of the general theorem spares us these efforts.

A construction, therefore, becomes interesting only when it can be ranged beside other analogous constructions, forming species of the same genus.

If the quadrilateral is something besides the juxtaposition of two triangles, this is because it belongs to the genus polygon.

Moreover, one must be able to demonstrate the properties of the genus without being forced to establish them successively for each of the species.

To attain that, we must necessarily mount from the particular to the general, ascending one or more steps.

The analytic procedure 'by construction' does not oblige us to descend, but it leaves us at the same level.

We can ascend only by mathematical induction, which alone can teach us something new. Without the aid of this induction, different in certain respects from physical induction, but quite as fertile, construction would be powerless to create science.

Observe finally that this induction is possible only if the same operation can be repeated indefinitely. That is why the theory of chess can never become a science, for the different moves of the same game do not resemble one another.

CHAPTER II.

MATHEMATICAL MAGNITUDE AND EXPERIENCE.

To learn what mathematicians understand by a continuum, one should not inquire of geometry. The geometer always seeks to represent to himself more or less the figures he studies, but his representations are for him only instruments; in making geometry he uses space just as he does chalk; so too much weight should not be attached to non-essentials, often of no more importance than the whiteness of the chalk.

The pure analyst has not this rock to fear. He has disengaged the science of mathematics from all foreign elements, and can answer our question: 'What exactly is this continuum about which mathematicians reason?' Many analysts who reflect on their art have answered already; Monsieur Tannery, for example, in his *Introduction à la théorie des fonctions d'une variable*.

Let us start from the scale of whole numbers; between two consecutive steps, intercalate one or more intermediary steps, then between these new steps still others, and so on indefinitely. Thus we shall have an unlimited number of terms; these will be the numbers called fractional, rational or commensurable. But this is not yet enough; between these terms, which, however, are already infinite in number, it is still necessary to intercalate others called irrational or incommensurable. A remark before going further. The continuum so conceived is only a collection of individuals ranged in a certain order, infinite in number, it is true, but *exterior* to one another. This is not the ordinary conception, wherein is supposed between the elements of the continuum a sort of intimate bond which makes of them a whole, where the point does not exist before the line, but the line before the point. Of the celebrated formula, 'the continuum is unity in multiplicity,' only the multiplicity remains, the unity has disappeared. The analysts are none the less right in defining their continuum as they do, for they always reason on just this as soon as they pique themselves on their rigor. But this is

enough to apprise us that the veritable mathematical continuum is a very different thing from that of the physicists and that of the metaphysicians.

It may also be said perhaps that the mathematicians who are content with this definition are dupes of words, that it is necessary to say precisely what each of these intermediary steps is, to explain how they are to be intercalated and to demonstrate that it is possible to do it. But that would be wrong; the only property of these steps which is used in their reasonings* is that of being before or after such and such steps; therefore also this alone should occur in the definition.

So how the intermediary terms should be intercalated need not concern us; on the other hand, no one will doubt the possibility of this operation, unless from forgetting that possible, in the language of geometers, simply means free from contradiction.

Our definition, however, is not yet complete, and I return to it after this over-long digression.

DEFINITION OF INCOMMENSURABLES.—The mathematicians of the Berlin school, Kronecker in particular, have devoted themselves to constructing this continuous scale of fractional and irrational numbers without using any material other than the whole number. The mathematical continuum would be, in this view, a pure creation of the mind, where experience would have no part.

The notion of the rational number seeming to them to present no difficulty, they have chiefly striven to define the incommensurable number. But before reproducing here their definition, I must make a remark to forestall the astonishment it is sure to arouse in readers unfamiliar with the customs of geometers.

Mathematicians study not objects, but relations between objects; the replacement of these objects by others is therefore indifferent to them, provided the relations do not change. The matter is for them unimportant, the form alone interests them.

Without recalling this, it would scarcely be comprehensible that Dedekind should designate by the name *incommensurable number* a mere symbol, that is to say, something very different from the ordinary idea of a quantity, which should be measurable and almost tangible.

* With those contained in the special conventions which serve to define addition and of which we shall speak later.

Let us see now what Dedekind's definition is:

The commensurable numbers can in an infinity of ways be partitioned into two classes, such that any number of the first class is greater than any number of the second class.

It may happen that among the numbers of the first class there is one smaller than all the others; if, for example, we range in the first class all numbers greater than 2, and 2 itself, and in the second class all numbers less than 2, it is clear that 2 will be the least of all numbers of the first class. The number 2 may be chosen as symbol of this partition.

It may happen, on the contrary, that among the numbers of the second class is one greater than all the others; this is the case, for example, if the first class comprehends all numbers greater than 2, and the second all numbers less than 2, and 2 itself. Here again the number 2 may be chosen as symbol of this partition.

But it may equally well happen that neither is there in the first class a number less than all the others, nor in the second class a number greater than all the others. Suppose, for example, we put in the first class all commensurable numbers whose squares are greater than 2 and in the second all whose squares are less than 2. There is none whose square is precisely 2. Evidently there is not in the first class a number less than all the others, for, however near the square of a number may be to 2, we can always find a commensurable number whose square is still closer to 2.

In Dedekind's view, the incommensurable number

$$\sqrt{2} \text{ or } (2)^{\frac{1}{2}}$$

is nothing but the symbol of this particular mode of partition of commensurable numbers; and to each mode of partition corresponds thus a number, commensurable or not, which serves as its symbol.

But to be content with this would be to forget too far the origin of these symbols; it remains to explain how we have been led to attribute to them a sort of concrete existence, and, besides, does not the difficulty begin even for the fractional numbers themselves? Should we have the notion of these numbers if we had

not previously known a matter that we conceive as infinitely divisible, that is to say, a continuum?

THE PHYSICAL CONTINUUM.—We ask ourselves then if the notion of the mathematical continuum is not simply drawn from experience. If it were, the raw data of experience, which are our sensations, would be susceptible of measurement. We might be tempted to believe they really are so, since in these latter days the attempt has been made to measure them and a law has even been formulated, known as Fechner's law, according to which sensation is proportional to the logarithm of the stimulus.

But if we examine more closely the experiments by which it has been sought to establish this law, we shall be led to a diametrically opposite conclusion. It has been observed, for example, that a weight *A* of 10 grams and a weight *B* of 11 grams produce identical sensations, that the weight *B* is just as indistinguishable from a weight *C* of 12 grams, but that the weight *A* is easily distinguished from the weight *C*. Thus the raw results of experience may be expressed by the following relations:

$$A=B, \quad B=C, \quad A < C,$$

which may be regarded as the formula of the physical continuum.

But here is an intolerable discord with the principle of contradiction, and the need of stopping this has compelled us to invent the mathematical continuum.

We are, therefore, forced to conclude that this notion has been created entirely by the mind, but that experience has given the occasion.

We can not believe that two quantities equal to a third are not equal to one another, and so we are led to suppose that *A* is different from *B* and *B* from *C*, but that the imperfection of our senses has not permitted of our distinguishing them.

CREATION OF THE MATHEMATICAL CONTINUUM.—*First Stage.* So far it would suffice, in accounting for the facts, to intercalate between *A* and *B* a few terms, which would remain discrete. What happens now if we have recourse to some instrument to supplement the feebleness of our senses, if, for example, we make use of a microscope? Terms such as *A* and *B*, before indistinguishable, appear now distinct; but between *A* and *B*, now be-

come distinct, will be intercalated a new term, D , that we can distinguish neither from A nor from B . Despite the employment of the most highly perfected methods, the raw results of our experience will always present the characteristics of the physical continuum with the contradiction which is inherent in it.

We shall escape it only by incessantly intercalating new terms between the terms already distinguished, and this operation must be continued indefinitely. We might conceive the stopping of this operation if we could imagine some instrument sufficiently powerful to decompose the physical continuum into discrete elements, as the telescope resolves the milky way into stars. But this we can not imagine; in fact, it is with the eye we observe the image magnified by the microscope, and consequently this image must always retain the characteristics of visual sensation and consequently those of the physical continuum.

Nothing distinguishes a length observed directly from the half of this length doubled by the microscope. The whole is homogeneous with the part; this is a new contradiction, or rather it would be if the number of terms were supposed finite; in fact, it is clear that the part containing fewer terms than the whole could not be similar to the whole.

The contradiction ceases when the number of terms is regarded as infinite; nothing hinders, for example, considering the aggregate of whole numbers as similar to the aggregate of even numbers, which, however, is only a part of it; and, in fact, to each whole number corresponds an even number, its double.

But it is not only to escape this contradiction contained in the empirical data that the mind is led to create the concept of a continuum, formed of an indefinite number of terms.

All happens as in the sequence of whole numbers. We have the faculty of conceiving that a unit can be added to a collection of units; thanks to experience, we have occasion to exercise this faculty and we become conscious of it; but from this moment we feel that our power has no limit and that we can count indefinitely, though we have never had to count more than a finite number of objects.

Just so, as soon as we have been led to intercalate means between two consecutive terms of a series, we feel that this opera-

tion can be continued beyond all limit, and that there is, so to speak, no intrinsic reason for stopping.

As an abbreviation, let me call a mathematical continuum of the first order every aggregate of terms formed according to the same law as the scale of commensurable numbers. If we afterwards intercalate new steps according to the law of formation of incommensurable numbers, we shall obtain what we will call a continuum of the second order.

Second Stage.—We have made hitherto only the first stride; we have explained the origin of continua of the first order; but it is necessary to see why even they are not sufficient and why the incommensurable numbers had to be invented.

If we try to imagine a line, it must have the characteristics of the physical continuum, that is to say, we shall not be able to represent it except with a certain breadth. Two lines then will appear to us under the form of two narrow bands, and, if we are content with this rough image, it is evident that if the two lines cross, they will have a common part.

But the pure geometer makes a further effort; without entirely renouncing the aid of the senses, he tries to reach the concept of the line without breadth, of the point without extension. This he can only attain to by regarding the line as the limit toward which tends an ever narrowing band, and the point as the limit toward which tends an ever lessening area. And then, our two bands, however narrow they may be, will always have a common area, the smaller as they are the narrower, and whose limit will be what the pure geometer calls a point.

This is why it is said two lines which cross have a point in common, and this truth seems intuitive.

But it would imply contradiction if lines were conceived as continua of the first order, that is to say, if on the lines traced by the geometer should be found only points having for coordinates rational numbers. The contradiction would be manifest as soon as one affirmed, for example, the existence of straights and circles.

It is clear, in fact, that if the points whose coordinates are commensurable were alone regarded as real, the circle inscribed in a square and the diagonal of this square would not intersect,

since the coordinates of the point of intersection are incommensurable.

That would not yet be sufficient, because we should get in this way only certain incommensurable numbers and not all those numbers.

But conceive of a straight line divided into two rays. Each of these rays will appear to our imagination as a band of a certain breadth; these bands moreover will encroach one on the other, since there must be no interval between them. The common part will appear to us as a point which will always remain when we try to imagine our bands narrower and narrower, so that we admit as an intuitive truth that if a straight is cut into two rays their common frontier is a point; we recognize here the conception of Dedekind, in which an incommensurable number was regarded as the common frontier of two classes of rational numbers.

Such is the origin of the continuum of the second order, which is the mathematical continuum properly so called.

Résumé.—In recapitulation, the mind has the faculty of creating symbols, and it is thus that it has constructed the mathematical continuum, which is only a particular system of symbols. Its power is limited only by the necessity of avoiding all contradiction; but the mind only makes use of this faculty if experience furnishes it a stimulus thereto.

In the case considered, this stimulus was the notion of the physical continuum, drawn from the rough data of the senses. But this notion leads to a series of contradictions from which it is necessary successively to free ourselves. So we are forced to imagine a more and more complicated system of symbols. That at which we stop is not only exempt from internal contradiction (it was so already at all the stages we have traversed), but neither is it in contradiction with various propositions called intuitive, which are derived from empirical notions more or less elaborated.

MEASURABLE MAGNITUDE.—The magnitudes we have studied hitherto are not *measurable*; we can indeed say whether a given one of these magnitudes is greater than another, but not whether it is twice or thrice as great.

So far, I have only considered the order in which our terms are ranged. But for most applications that does not suffice. We

must learn to compare the interval which separates any two terms. Only on this condition does the continuum become a measurable magnitude and the operations of arithmetic applicable.

This can only be done by the aid of a new and special *convention*. We will agree that in such and such a case the interval comprised between the terms A and B is equal to the interval which separates C and D . For example, at the beginning of our work we have set out from the scale of the whole numbers and we have supposed intercalated between two consecutive steps n intermediary steps; well, these new steps will be by convention regarded as equidistant.

This is a way of defining the addition of two magnitudes, because if the interval AB is by definition equal to the interval CD , the interval AD will be by definition the sum of the intervals AB and AC .

This definition is arbitrary in a very large measure. It is not completely so, however. It is subjected to certain conditions and, for example, to the rules of commutativity and associativity of addition. But provided the definition chosen satisfies these rules, the choice is indifferent, and it is useless to particularize it.

VARIOUS REMARKS.—We can now discuss several important questions:

1° Is the creative power of the mind exhausted by the creation of the mathematical continuum?

No, the works of Du Bois-Reymond demonstrate it in a striking way.

We know that mathematicians distinguish between infinitesimals of different orders and that those of the second order are infinitesimal, not only in an absolute way, but also in relation to those of the first order. It is not difficult to imagine infinitesimals of fractional or even of irrational order, and thus we find again that scale of the mathematical continuum which has been dealt with in the preceding pages.

Further, there are infinitesimals which are infinitely small in relation to those of the first order, and, on the contrary, infinitely great in relation to those of order $1 + \epsilon$, and that however small ϵ may be. Here, then, are new terms intercalated in our series, and if I may be permitted to revert to the phraseology lately em-

ployed which is very convenient though not consecrated by usage, I shall say that thus has been created a sort of continuum of the third order.

It would be easy to go further, but that would be idle; one would only be imagining symbols without possible application, and no one will think of doing that. The continuum of the third order, to which the consideration of the different orders of infinitesimals leads, is itself not useful enough to have won citizenship, and geometers regard it only as a mere curiosity. The mind uses its creative faculty only when experience requires it.

2° Once in possession of the concept of the mathematical continuum, is one safe from contradictions analogous to those which gave birth to it?

No, and I will give an example.

One must be very wise not to regard it as evident that every curve has a tangent; and in fact if we picture this curve and a straight as two narrow bands we can always so dispose them that they have a part in common without crossing. If we imagine then the breadth of these two bands to diminish indefinitely, this common part will always subsist and, at the limit, so to speak, the two lines will have a point in common without crossing, that is to say, they will be tangent.

The geometer who reasons in this way, consciously or not, is only doing what we have done above to prove two lines which cut have a point in common, and his intuition might seem just as legitimate.

It would deceive him however. We can demonstrate that there are curves which have no tangent, if such a curve is defined as an analytic continuum of the second order.

Without doubt some artifice analogous to those we have discussed above would have sufficed to remove the contradiction; but, as this is met with only in very exceptional cases, it has received no further attention.

Instead of seeking to reconcile intuition with analysis, we have been content to sacrifice one of the two, and as analysis must remain impeccable, we have decided against intuition.

THE PHYSICAL CONTINUUM OF SEVERAL DIMENSIONS.—We have discussed above the physical continuum as derived from the

immediate data of our senses, or, if you wish, from the rough results of Fechner's experiments; I have shown that these results are summed up in the contradictory formulas

$$A = B, \quad B = C, \quad A < C.$$

Let us now see how this notion has been generalized and how from it has come the concept of many-dimensional continua.

Consider any two aggregates of sensations. Either we can discriminate them one from another, or we can not, just as in Fechner's experiments a weight of 10 grams can be distinguished from a weight of 12 grams, but not from a weight of 11 grams. This is all that is required to construct the continuum of several dimensions.

Let us call one of these aggregates of sensations an *element*. That will be something analogous to the *point* of the mathematicians; it will not be altogether the same thing however. We can not say our element is without extension, since we can not distinguish it from neighboring elements and it is thus surrounded by a sort of haze. If the astronomical comparison may be allowed, our 'elements' would be like nebulae, whereas the mathematical points would be like stars.

That being granted, a system of elements will form a *continuum* if we can pass from any one of them to any other, by a series of consecutive elements such that each is indistinguishable from the preceding. This *linear series* is to the *line* of the mathematician what an isolated *element* was to the point.

Before going farther, I must explain what is meant by a *cut*. Consider a continuum *C* and remove from it certain of its elements which for an instant we shall regard as no longer belonging to this continuum. The aggregate of the elements so removed will be called a cut. It may happen that thanks to this cut, *C* may be *subdivided* into several distinct continua, the aggregate of the remaining elements ceasing to form a unique continuum.

There will then be on *C* two elements, *A* and *B*, that must be regarded as belonging to two distinct continua, and this will be recognized because it will be impossible to find a linear series of consecutive elements of *C*, each of these elements indistinguish-

able from the preceding, the first being A and the last B , *without one of the elements of this series being indistinguishable from one of the elements of the cut.*

On the contrary, it may happen that the cut made is insufficient to subdivide the continuum C . To classify the physical continua, we will examine precisely what are the cuts which must be made to subdivide them.

If a physical continuum C can be subdivided by a cut reducing to a finite number of elements all distinguishable from one another (and consequently forming neither a continuum, nor several continua), we shall say C is a *one-dimensional* continuum.

If, on the contrary, C can be subdivided only by cuts which are themselves continua, we shall say C has several dimensions. If cuts which are continua of one dimension suffice, we shall say C has two dimensions; if cuts of two dimensions suffice, we shall say C has three dimensions, and so on.

Thus is defined the notion of the physical continuum of several dimensions, thanks to this very simple fact that two aggregates of sensations are distinguishable or indistinguishable.

THE MATHEMATICAL CONTINUUM OF SEVERAL DIMENSIONS.—Thence the notion of the mathematical continuum of n dimensions has sprung quite naturally by a process very like that we discussed at the beginning of this chapter. A point of such a continuum, you know, appears to us as defined by a system of n distinct magnitudes called its coordinates.

These magnitudes need not always be measurable; there is, for instance, a branch of geometry independent of the measurement of these magnitudes, in which it is only a question of knowing, for example, whether on a curve ABC , the point B is between the points A and C , and not of knowing whether the arc AB is equal to the arc BC or twice as great. This is what is called *Analysis Situs*.

This is a whole body of doctrine which has attracted the attention of the greatest geometers and where we see flow one from another a series of remarkable theorems. What distinguishes these theorems from those of ordinary geometry is that they are purely qualitative and that they would remain true if the figures

were copied by a draughtsman so awkward as to grossly distort the proportions and replace straights by strokes more or less curved.

Through the wish to introduce measure next into the continuum just defined this continuum becomes space, and geometry is born. But the discussion of this is reserved for Part Second.

PART II.

SPACE.

CHAPTER III.

THE NON-EUCLIDEAN GEOMETRIES.

EVERY conclusion supposes premises; these premises themselves either are self-evident and need no demonstration, or can be established only by relying upon other propositions, and since we can not go back thus to infinity, every deductive science, and in particular geometry, must rest on a certain number of undemonstrable axioms. All treatises on geometry begin, therefore, by the enunciation of these axioms. But among these there is a distinction to be made: Some, for example, 'Things which are equal to the same thing are equal to one another,' are not propositions of geometry, but propositions of analysis. I regard them as analytic judgments *a priori*, and shall not concern myself with them.

But I must lay stress upon other axioms which are peculiar to geometry. Most treatises enunciate three of these explicitly:

- 1° Through two points can pass only one straight;
- 2° The straight line is the shortest path from one point to another;
- 3° Through a given point there is not more than one parallel to a given straight.

Although generally a proof of the second of these axioms is omitted, it would be possible to deduce it from the other two and from those, much more numerous, which are implicitly admitted without enunciating them, as I shall explain further on.

It was long sought in vain to demonstrate likewise the third axiom, known as *Euclid's Postulate*. What vast effort has been wasted in this chimeric hope is truly unimaginable. Finally, in the first quarter of the nineteenth century, and almost at the same

time, a Hungarian and a Russian, Bolyai and Lobachevski, established irrefutably that this demonstration is impossible; they have almost rid us of inventors of geometries 'sans postulat'; since then the Académie des Sciences receives only about one or two new demonstrations a year.

The question was not exhausted; it soon made a great stride by the publication of Riemann's celebrated memoir entitled: *Ueber die Hypothesen welche der Geometrie zu Grunde liegen*. This paper has inspired most of the recent works of which I shall speak further on, and among which it is proper to cite those of Beltrami and of Helmholtz.

THE BOLYAI-LOBACHEVSKI GEOMETRY.—If it were possible to deduce Euclid's postulate from the other axioms, it is evident that in denying the postulate and admitting the other axioms, we should be led to contradictory consequences; it would therefore be impossible to base on such premises a coherent geometry.

Now this is precisely what Lobachevski did.

He assumes at the start that: *Through a given point can be drawn two parallels to a given straight.*

And he retains besides all Euclid's other axioms. From these hypotheses he deduces a series of theorems among which it is impossible to find any contradiction, and he constructs a geometry whose faultless logic is inferior in nothing to that of the Euclidean geometry.

The theorems are, of course, very different from those to which we are accustomed, and they can not fail to be at first a little disconcerting.

Thus the sum of the angles of a triangle is always less than two right angles, and the difference between this sum and two right angles is proportional to the surface of the triangle.

It is impossible to construct a figure similar to a given figure but of different dimensions.

If we divide a circumference into n equal parts, and draw tangents at the points of division, these n tangents will form a polygon if the radius of the circle is small enough; but if this radius is sufficiently great they will not meet.

It is useless to multiply these examples; Lobachevski's propositions have no relation to those of Euclid, but they are not less logically bound one to another.

RIEMANN'S GEOMETRY.—Imagine a world uniquely peopled by beings of no thickness (height); and suppose these 'infinitely flat' animals are all in the same plane and can not get out. Admit besides that this world is sufficiently far from others to be free from their influence. While we are making hypotheses, it costs us no more to endow these beings with reason and believe them capable of creating a geometry. In that case, they will certainly attribute to space only two dimensions.

But suppose now that these imaginary animals, while remaining without thickness, have the form of a spherical, and not of a plane figure, and are all on the same sphere without power to get off. What geometry will they construct? First it is clear they will attribute to space only two dimensions; what will play for them the rôle of the straight line will be the shortest path from one point to another on the sphere, that is to say an arc of a great circle; in a word, their geometry will be the spherical geometry.

What they will call space will be this sphere on which they must stay, and on which happen all the phenomena they can know. Their space will therefore be *unbounded* since on a sphere one can always go forward without ever being stopped, and yet it will be *finite*; one can never find the end of it, but one can make a tour of it.

Well, Riemann's geometry is spherical geometry extended to three dimensions. To construct it, the German mathematician had to throw overboard, not only Euclid's postulate, but also the first axiom: *Only one straight can pass through two points.*

On a sphere, through two given points we can draw *in general* only one great circle (which, as we have just seen, would play the rôle of the straight for our imaginary beings); but there is an exception: if the two given points are diametrically opposite, an infinity of great circles can be drawn through them.

In the same way, in Riemann's geometry (at least in one of its forms), through two points will pass in general only a single straight; but there are exceptional cases where through two points an infinity of straights can pass.

There is a sort of opposition between Riemann's geometry and that of Lobachevski.

Thus the sum of the angles of a triangle is:

Equal to two right angles in Euclid's geometry;
 Less than two right angles in that of Lobachevski;
 Greater than two right angles in that of Riemann.

The number of straights through a given point that can be drawn coplanar to a given straight, but nowhere meeting it, is equal:

To one in Euclid's geometry;
 To zero in that of Riemann;
 To infinity in that of Lobachevski.

Add that Riemann's space is finite, although unbounded in the sense given above to these two words.

THE SURFACES OF CONSTANT CURVATURE.—One objection still remained possible. The theorems of Lobachevski and of Riemann present no contradiction; but however numerous the consequences these two geometers have drawn from their hypotheses, they must have stopped before exhausting them, since their number would be infinite; who can say then that if they had pushed their deductions farther they would not have eventually reached some contradiction?

This difficulty does not exist for Riemann's geometry, provided it is limited to two dimensions; in fact, as we have seen, two-dimensional Riemannian geometry does not differ from spherical geometry, which is only a branch of ordinary geometry, and consequently is beyond all discussion.

Beltrami, in correlating likewise Lobachevski's two-dimensional geometry with a branch of ordinary geometry, has equally refuted the objection so far as it is concerned.

Here is how he accomplished it. Consider any figure on a surface. Imagine this figure traced on a flexible and inextensible canvas applied over this surface in such a way that when the canvas is displaced and deformed, the various lines of this figure can change their form without changing their length. In general, this flexible and inextensible figure can not be displaced without leaving the surface; but there are certain particular surfaces for which such a movement would be possible; these are the surfaces of constant curvature.

If we resume the comparison made above and imagine beings without thickness living on one of these surfaces, they will regard

as possible the motion of a figure all of whose lines remain constant in length. On the contrary, such a movement would appear absurd to animals without thickness living on a surface of variable curvature.

These surfaces of constant curvature are of two sorts: Some are of *positive curvature*, and can be deformed so as to be applied over a sphere. The geometry of these surfaces reduces itself therefore to the spherical geometry, which is that of Riemann.

The others are of *negative curvature*. Beltrami has shown that the geometry of these surfaces is none other than that of Lobachevski. The two-dimensional geometries of Riemann and Lobachevski are thus correlated to the Euclidean geometry.

INTERPRETATION OF NON-EUCLIDEAN GEOMETRIES.—So vanishes the objection so far as two-dimensional geometries are concerned.

It would be easy to extend Beltrami's reasoning to three-dimensional geometries. The minds that space of four dimensions does not repel will see no difficulty in it, but they are few. I prefer therefore to proceed otherwise.

Consider a certain plane, which I shall call the fundamental plane, and construct a sort of dictionary, by making correspond each to each a double series of terms written in two columns, just as correspond in the ordinary dictionaries the words of two languages whose signification is the same:

Space: Portion of space situated above the fundamental plane.

Plane: Sphere cutting the fundamental plane orthogonally.

Straight: Circle cutting the fundamental plane orthogonally.

Sphere: Sphere.

Circle: Circle.

Angle: Angle.

Distance between two points: Logarithm of the cross ratio of these two points and the intersections of the fundamental plane with a circle passing through these two points and cutting it orthogonally.

Etc., Etc.

Now take Lobachevski's theorems and translate them with the aid of this dictionary as we translate a German text with the aid of a German-English dictionary. *We shall thus obtain the-*

orems of the ordinary geometry. For example, that theorem of Lobachevski: 'the sum of the angles of a triangle is less than two right angles' is translated thus: "If a curvilinear triangle has for sides circle-arcs which prolonged would cut orthogonally the fundamental plane, the sum of the angles of this curvilinear triangle will be less than two right angles." Thus, however far the consequences of Lobachevski's hypotheses are pushed, they will never lead to a contradiction. In fact, if two of Lobachevski's theorems were contradictory, it would be the same with the translations of these two theorems, made by the aid of our dictionary, but these translations are theorems of ordinary geometry and no one doubts that the ordinary geometry is free from contradiction. Whence comes this certainty and is it justified? That is a question I can not treat here because it would require to be enlarged upon, but which is very interesting and I think not insoluble.

Nothing remains then of the objection above formulated. This is not all. Lobachevski's geometry, susceptible of a concrete interpretation, ceases to be a vain logical exercise and is capable of applications; I have not the time to speak here of these applications, nor of the aid that Klein and I have gotten from them for the integration of linear differential equations.

This interpretation moreover is not unique, and several dictionaries analogous to the preceding could be constructed, which would enable us by a simple 'translation' to transform Lobachevski's theorems into theorems of ordinary geometry.

THE IMPLICIT AXIOMS.—Are the axioms explicitly enunciated in our treatises the sole foundations of geometry? We may be assured of the contrary by noticing that after they are successively abandoned there are still left over some propositions common to the theories of Euclid, Lobachevski and Riemann. These propositions must rest on premises the geometers admit without enunciation. It is interesting to try to disentangle them from the classic demonstrations.

Stuart Mill has claimed that every definition contains an axiom, because in defining one affirms implicitly the existence of the object defined. This is going much too far; it is rare that in mathematics a definition is given without its being followed by the demonstration of the existence of the object defined, and when

this is dispensed with it is generally because the reader can easily supply it. It must not be forgotten that the word existence has not the same sense when it refers to a mathematical entity and when it is a question of a material object. A mathematical entity exists, provided its definition implies no contradiction, either in itself, or with the propositions already admitted.

But if Stuart Mill's observation can not be applied to all definitions, it is none the less just for some of them. The plane is sometimes defined as follows:

The plane is a surface such that the straight which joins any two of its points is wholly on this surface.

This definition manifestly hides a new axiom; it is true we might change it, and that would be preferable, but then we should have to enunciate the axiom explicitly.

Other definitions would suggest reflections not less important.

Such, for example, is that of the equality of two figures; two figures are equal when they can be superposed; to superpose them one must be displaced until it coincides with the other; but how shall it be displaced? If we should ask this, no doubt we should be told that it must be done without altering the shape and as a rigid solid. The vicious circle would then be evident.

In fact this definition defines nothing; it would have no meaning for a being living in a world where there were only fluids. If it seems clear to us, that is because we are used to the properties of natural solids which do not differ much from those of the ideal solids, all of whose dimensions are invariable.

Yet, imperfect as it may be, this definition implies an axiom.

The possibility of the motion of a rigid figure is not a self-evident truth, or at least it is so only in the fashion of Euclid's postulate and not as an analytic judgment *a priori* would be.

Moreover, in studying the definitions and the demonstrations of geometry, we see that one is obliged to admit without proof not only the possibility of this motion, but some of its properties besides.

This is at once seen from the definition of the straight line. Many defective definitions have been given, but the true one is that which is implied in all the demonstrations where the straight line enters:

"It may happen that the motion of a rigid figure is such that all the points of a line belonging to this figure remain motionless while all the points situated outside of this line move. Such a line will be called a straight line." We have designedly, in this enunciation, separated the definition from the axiom it implies.

Many demonstrations, such as those of the cases of the equality of triangles, of the possibility of dropping a perpendicular from a point to a straight, presume propositions which are not enunciated, for they require the admission that it is possible to transport a figure in a certain way in space.

THE FOURTH GEOMETRY.—Among these implicit axioms, there is one which seems to me to merit some attention, because when it is abandoned, a fourth geometry can be constructed as coherent as those of Euclid, Lobachevski and Riemann.

To prove that a perpendicular may always be erected at a point A to a straight AB , we consider a straight AC movable around the point A and initially coincident with the fixed straight AB ; and we make it turn about the point A until it comes into the prolongation of AB .

Thus two propositions are presupposed: First, that such a rotation is possible, and next that it may be continued until the two straights come into the prolongation one of the other.

If the first point is admitted and the second rejected, we are led to a series of theorems even stranger than those of Lobachevski and Riemann, but equally exempt from contradiction.

I shall cite only one of these theorems and that not the most singular: *A real straight may be perpendicular to itself.*

LIE'S THEOREM.—The numbers of axioms implicitly introduced in the classic demonstrations is greater than necessary, and it would be interesting to reduce it to a minimum. It may first be asked whether this reduction is possible, whether the number of necessary axioms and that of imaginable geometries are not infinite.

A theorem of Sophus Lie dominates this whole discussion. It may be thus enunciated:

Suppose the following premises are admitted:

- 1° Space has n dimensions;
- 2° The motion of a rigid figure is possible;

3° It requires p conditions to determine the position of this figure in space.

The number of geometries compatible with these premises will be limited.

I may even add that if n is given, a superior limit can be assigned to p .

If therefore the possibility of motion is admitted, there can be invented only a finite (and even a rather small) number of three-dimensional geometries.

RIEMANN'S GEOMETRIES.—Yet this result seems contradicted by Riemann, for this savant constructs an infinity of different geometries, and that to which his name is ordinarily given is only a particular case.

All depends, he says, on how the length of a curve is defined. Now, there is an infinity of ways of defining this length, and each of them may be the starting point of a new geometry.

That is perfectly true, but most of these definitions are incompatible with the motion of a rigid figure, which in the theorem of Lie is supposed possible. These geometries of Riemann, in many ways so interesting, could never therefore be other than purely analytic and would not lend themselves to demonstrations analogous to those of Euclid.

ON THE NATURE OF AXIOMS.—Most mathematicians regard Lobachevski's geometry only as a mere logical curiosity; some of them, however, have gone farther. Since several geometries are possible, is it certain ours is the true one? Experience no doubt teaches us that the sum of the angles of a triangle is equal to two right angles; but this is because the triangles we deal with are too little; the difference, according to Lobachevski, is proportional to the surface of the triangle; will it not perhaps become sensible when we shall operate on larger triangles or when our measurements shall become more precise? The Euclidean geometry would thus be only a provisional geometry.

To discuss this opinion, we should first ask ourselves what is the nature of the geometric axioms.

Are they synthetic *a priori* judgments, as Kant said?

They would then impose themselves upon us with such force, that we could not conceive the contrary proposition, nor build upon it a theoretic edifice. There would be no non-Euclidean geometry.

To be convinced of it take a veritable synthetic *a priori* judgment, the following, for instance, of which we have seen the preponderant rôle in the first chapter:

If a theorem is true for the number 1, and if it has been proved that it is true of $n + 1$ provided it is true of n , it will be true of all the positive whole numbers.

Then try to escape from that and, denying this proposition, try to found a false arithmetic analogous to non-Euclidean geometry—it can not be done; one would even be tempted at first blush to regard these judgments as analytic.

Moreover, resuming our fiction of animals without thickness, we can hardly admit that these beings, if their minds are like ours, would adopt the Euclidean geometry which would be contradicted by all their experience.

Should we therefore conclude that the axioms of geometry are experimental verities? But we do not experiment on ideal straight lines or circles; it can only be done on material objects. On what then could be based experiments which should serve as foundation for geometry? The answer is easy.

We have seen above that we constantly reason as if the geometric figures behaved like solids. What geometry would borrow from experience would therefore be the properties of these bodies. The properties of light and its rectilinear propagation have also given rise to some of the propositions of geometry, and in particular those of projective geometry, so that from this point of view one would be tempted to say that metric geometry is the study of solids, and projective, that of light.

But a difficulty remains, and it is insurmountable. If geometry were an experimental science, it would not be an exact science, it would be subject to a continual revision. Nay, it would from this very day be convicted of error, since we know that there is no rigorously rigid solid.

The axioms of geometry therefore are neither synthetic a priori judgments nor experimental facts.

They are *conventions*; our choice among all possible conventions is *guided* by experimental facts; but it remains *free* and is limited only by the necessity of avoiding all contradiction. Thus it is that the postulates can remain *rigorously* true even though

the experimental laws which have determined their adoption are only approximative.

In other words, *the axioms of geometry* (I do not speak of those of arithmetic) *are merely disguised definitions*.

Then what are we to think of that question: Is the Euclidean geometry true?

It has no meaning.

As well ask whether the metric system is true and the old measures false; whether Cartesian coordinates are true and polar coordinates false. One geometry can not be more true than another; it can only be *more convenient*.

Now, Euclidean geometry is, and will remain, the most convenient:

1° Because it is the simplest; and it is so not only in consequence of our mental habits, or of I know not what direct intuition that we may have of Euclidean space; it is the simplest in itself, just as a polynomial of the first degree is simpler than one of the second; the formulas of spherical trigonometry are more complicated than those of plane trigonometry, and they would still appear so to an analyst ignorant of their geometric signification.

2° Because it accords sufficiently well with the properties of natural solids, those bodies which our hands and our eye compare and with which we make our instruments of measure.

CHAPTER IV.

SPACE AND GEOMETRY.

LET us begin by a little paradox.

Beings with minds like ours, and having the same senses as we, but without previous education, would receive from a suitably chosen external world impressions such that they would be led to construct a geometry other than that of Euclid and to localize the phenomena of that external world in a non-Euclidean space, or even in a space of four dimensions.

As for us, whose education has been accomplished by our actual world, if we were suddenly transported into this new world, we should have no difficulty in referring its phenomena to our Euclidean space. Conversely, if these beings were transported into our environment, they would be led to relate our phenomena to non-Euclidean space.

Nay more; with a little effort we likewise could do it. A person who should devote his existence to it might perhaps attain to a realization of the fourth dimension.

GEOMETRIC SPACE AND PERCEPTUAL SPACE.—It is often said the images of external objects are localized in space, even that they can not be formed except on this condition. It is also said that this space, which serves thus as a ready prepared *frame* for our sensations and our representations, is identical with that of the geometers, of which it possesses all the properties.

To all the good minds who think thus, the preceding statement must have appeared quite extraordinary. But let us see whether they are not subject to an illusion that a more profound analysis would dissipate.

What, first of all, are the properties of space, properly so called? I mean of that space which is the object of geometry and which I shall call *geometric space*.

The following are some of the most essential:

- 1° It is continuous;
- 2° It is infinite;

3° It has three dimensions;

4° It is homogeneous, that is to say, all its points are identical one with another;

5° It is isotropic, that is to say, all the straights which pass through the same point are identical one with another.

Compare it now to the frame of our representations and our sensations, which I may call *perceptual space*.

VISUAL SPACE.—Consider first a purely visual impression, due to an image formed on the bottom of the retina.

A cursory analysis shows us this image as continuous, but as possessing only two dimensions; this already distinguishes from geometric space what we may call *pure visual space*.

Besides, this image is enclosed in a limited frame.

Finally, there is another difference not less important: *this pure visual space is not homogeneous*. All the points of the retina, aside from the images which may there be formed, do not play the same rôle. The yellow spot can in no way be regarded as identical with a point on the border of the retina. In fact, not only does the same object produce there much more vivid impressions, but in every *limited* frame the point occupying the center of the frame will never appear as equivalent to a point near one of the borders.

No doubt a more profound analysis would show us that this continuity of visual space and its two dimensions are only an illusion; it would separate it therefore still more from geometric space, but we shall not dwell on this remark.

Sight, however, enables us to judge of distances and consequently to perceive a third dimension. But every one knows that this perception of the third dimension reduces itself to the sensation of the effort at accommodation it is necessary to make, and to that of the convergence which must be given to the two eyes, to perceive an object distinctly.

These are muscular sensations altogether different from the visual sensations which have given us the notion of the first two dimensions. The third dimension therefore will not appear to us as playing the same rôle as the other two. What may be called *complete visual space* is therefore not an isotropic space.

It has, it is true, precisely three dimensions, which means

that the elements of our visual sensations (those at least which combine to form the notion of extension) will be completely defined when three of them are known; to use the language of mathematics, they will be functions of three independent variables.

But examine the matter a little more closely. The third dimension is revealed to us in two different ways: by the effort of accommodation and by the convergence of the eyes.

No doubt these two indications are always concordant, there is a constant relation between them, or in mathematical terms, the two variables which measure these two muscular sensations do not appear to us as independent; or again, to avoid an appeal to mathematical notions already rather refined, we may go back to the language of the preceding chapter and enunciate the same fact as follows: If two sensations of convergence, A and B , are indistinguishable, the two sensations of accommodation, A' and B' , which respectively accompany them, will be equally indistinguishable.

But here we have, so to speak, an experimental fact; *a priori* nothing prevents our supposing the contrary, and if the contrary takes place, if these two muscular sensations vary independently of one another, we shall have to take account of one more independent variable, and 'complete visual space' will appear to us as a physical continuum of four dimensions.

We have here even, I will add, a fact of *external* experience. Nothing prevents our supposing that a being with a mind like ours, having the same sense organs that we have, may be placed in a world where light would only reach him after having traversed reflecting media of complicated form. The two indications which serve us in judging distances would cease to be connected by a constant relation. A being who should achieve in such a world the education of his senses would no doubt attribute four dimensions to complete visual space.

TACTILE SPACE AND MOTOR SPACE.—'Tactile space' is still more complicated than visual space and farther removed from geometric space. It is superfluous to repeat for touch the discussion I have given for sight.

But apart from the data of sight and touch, there are other sensations which contribute as much and more than they to the

genesis of the notion of space. These are known to every one; they accompany all our movements, and are usually called muscular sensations.

The corresponding frame constitutes what may be called *motor space*.

Each muscle gives rise to a special sensation capable of augmenting or of diminishing, so that the totality of our muscular sensations will depend upon as many variables as we have muscles. From this point of view, *motor space would have as many dimensions as we have muscles*.

I know it will be said that if the muscular sensations contribute to form the notion of space, it is because we have the sense of the *direction* of each movement and that it makes an integrant part of the sensation. If this were so, if a muscular sensation could not arise except accompanied by this geometric sense of direction, geometric space would indeed be a form imposed upon our sensibility.

But I perceive nothing at all of this when I analyze my sensations.

What I do see is that the sensations which correspond to movements in the same direction are connected in my mind by a mere *association of ideas*. It is to this association that what we call 'the sense of direction' is reducible. This feeling therefore can not be found in a single sensation.

This association is extremely complex, for the contraction of the same muscle may correspond, according to the position of the limbs, to movements of very different direction.

Besides, it is evidently acquired; it is, like all associations of ideas, the result of a *habit*; this habit itself results from very numerous *experiences*; without any doubt, if the education of our senses had been accomplished in a different environment, where we should have been subjected to different impressions, contrary habits would have arisen and our muscular sensations would have been associated according to other laws.

CHARACTERISTICS OF PERCEPTUAL SPACE.—Thus perceptual space, under its triple form, visual, tactile and motor, is essentially different from geometric space.

It is neither homogeneous, nor isotropic; one can not even say that it has three dimensions.

It is often said that we 'project' into geometric space the objects of our external perception; that we 'localize' them.

Has this a meaning, and if so what?

Does it mean that we *represent* to ourselves external objects in geometric space?

Our representations are only the reproduction of our sensations; they can therefore be ranged only in the same frame as these, that is to say, in perceptual space.

It is as impossible for us to represent to ourselves external bodies in geometric space, as it is for a painter to paint on a plane canvas objects with their three dimensions.

Perceptual space is only an image of geometric space, an image altered in shape by a sort of perspective, and we can represent to ourselves objects only by bringing them under the laws of this perspective.

Therefore we do not *represent* to ourselves external bodies in geometric space, but we *reason* on these bodies as if they were situated in geometric space.

When it is said then that we 'localize' such and such an object at such and such a point of space, what does it mean?

It simply means that we represent to ourselves the movements it would be necessary to make to reach that object; and one may not say that to represent to oneself these movements, it is necessary to project the movements themselves in space and that the notion of space must, consequently, pre-exist.

When I say that we represent to ourselves these movements, I mean only that we represent to ourselves the muscular sensations which accompany them and which have no geometric character whatever, which consequently do not at all imply the pre-existence of the notion of space.

CHANGE OF STATE AND CHANGE OF POSITION.—But, it will be said, if the idea of geometric space is not imposed upon our mind, and if, on the other hand, none of our sensations can furnish it, how could it have come into existence?

This is what we have now to examine, and it will take some time, but I can summarize in a few words the attempt at explanation that I am about to develop.

None of our sensations, isolated, could have conducted us to

the idea of space; we are led to it only in studying the laws according to which these sensations succeed each other.

We see first that our impressions are subject to change; but among the changes we ascertain we are soon led to make a distinction.

At one time we say that the objects which cause these impressions have changed state, at another time that they have changed position, that they have only been displaced.

Whether an object changes its state or merely its position, this is always translated for us in the same manner: *by a modification in an aggregate of impressions.*

How then could we have been led to distinguish between the two? It is easy to account for. If there has only been a change of position, we can restore the primitive aggregate of impressions by making movements which replace us opposite the mobile object in the same *relative* situation. We thus *correct* the modification that happened and we reestablish the initial state by an inverse modification.

If it is a question of sight, for example and if an object changes its place before our eye, we can 'follow it with the eye' and maintain its image on the same point of the retina by appropriate movements of the eyeball.

These movements we are conscious of because they are voluntary and because they are accompanied by muscular sensations, but that does not mean that we represent them to ourselves in geometric space.

So what characterizes change of position, what distinguishes it from change of state, is that it can always be corrected in this way.

It may therefore happen that we pass from the totality of impressions *A* to the totality *B* in two different ways:

1° Involuntarily and without experiencing muscular sensations; this happens when it is the object which changes place;

2° Voluntarily and with muscular sensations; this happens when the object is motionless, but we move so that the object has relative motion with reference to us.

If this be so, the passage from the totality *A* to the totality *B* is only a change of position.

It follows from this that sight and touch could not have given us the notion of space without the aid of the 'muscular sense.'

Not only could this notion not be derived from a single sensation or even from a series of sensations, but what is more, an *immobile* being could never have acquired it, since, not being able to *correct* by his movements the effects of the changes of position of exterior objects, he would have had no reason whatever to distinguish them from changes of state. Just as little could he have acquired it if his motions had not been voluntary or were unaccompanied by any sensations.

CONDITIONS OF COMPENSATION.—How is a like compensation possible, of such sort that two changes, otherwise independent of one another, reciprocally correct each other?

A mind already familiar with geometry would reason as follows: Evidently, if there is to be compensation, the various parts of the external object, on the one hand, and the various sense organs, on the other hand, must be in the same *relative* position after the double change. And, for that to be the case, the various parts of the external object must likewise have retained in reference to each other the same relative position, and the same must be true of the various parts of our body in regard to each other.

In other words, the external object, in the first change, must be displaced as is a rigid solid, and so must it be with the whole of our body in the second change which corrects the first.

Under these conditions, compensation may take place.

But we who as yet know nothing of geometry, since for us the notion of space is not yet formed, we can not reason thus, we can not foresee *a priori* whether compensation is possible. But experience teaches us that it sometimes happens, and it is from this experimental fact that we start to distinguish changes of state from changes of position.

SOLID BODIES AND GEOMETRY.—Among surrounding objects there are some which frequently undergo displacements susceptible of being thus corrected by a *correlative* movement of our own body; these are the *solid bodies*. The other objects, whose form is variable, only exceptionally undergo like displacements (change of position without change of form). When a body changes its place *and its shape*, we can no longer, by appropriate movements,

bring back our sense-organs into the same *relative* situation with regard to this body; consequently we can no longer reestablish the primitive totality of impressions.

It is only later, and as a consequence of new experiences, that we learn how to decompose the bodies of variable form into smaller elements, such that each is displaced almost in accordance with the same laws as solid bodies. Thus we distinguish 'deformations' from other changes of state; in these deformations, each element undergoes a mere change of position, which can be corrected, but the modification undergone by the aggregate is more profound and is no longer susceptible of correction by a correlative movement.

Such a notion is already very complex and must have been relatively late in appearing; moreover it could not have arisen if the observation of solid bodies had not already taught us to distinguish changes of position.

Therefore, if there were no solid bodies in nature, there would be no geometry.

Another remark also deserves a moment's attention. Suppose a solid body to occupy successively the positions α and β ; in its first position, it will produce on us the totality of impressions A , and in its second position the totality of impressions B . Let there be now a second solid body, having qualities entirely different from the first, for example, a different color. Suppose it to pass from the position α , where it gives us the totality of impressions A' , to the position β , where it gives the totality of impressions B' .

In general, the totality A will have nothing in common with the totality A' , nor the totality B with the totality B' . The transition from the totality A to the totality B and that from the totality A' to the totality B' are therefore two changes which *in themselves* have in general nothing in common.

And yet we regard these two changes both as displacements and, furthermore, we consider them as the *same* displacement. How can that be?

It is simply because they can both be corrected by the *same* correlative movement of our body.

'Correlative movement' therefore constitutes the *sole con-*

nection between two phenomena which otherwise we never should have dreamt of likening.

On the other hand, our body, thanks to the number of its articulations and muscles, may make a multitude of different movements; but all are not capable of 'correcting' a modification of external objects; only those will be capable of it in which our whole body, or at least all those of our sense-organs which come into play, are displaced as a whole, that is, without their relative positions varying, or in the fashion of a solid body.

To summarize:

1° We are led at first to distinguish two categories of phenomena:

Some, involuntary, unaccompanied by muscular sensations, are attributed by us to external objects; these are external changes;

Others, opposite in character and attributed by us to the movements of our own body, are internal changes;

2° We notice that certain changes of each of these categories may be corrected by a correlative change of the other category;

3° We distinguish among external changes, those which have thus a correlative in the other category; these we call displacements; and just so among the internal changes, we distinguish those which have a correlative in the first category.

Thus are defined, thanks to this reciprocity, a particular class of phenomena which we call displacements.

The laws of these phenomena constitute the object of geometry.

LAW OF HOMOGENEITY.—The first of these laws is the law of homogeneity.

Suppose that, by an external change α , we pass from the totality of impressions A to the totality B , then that this change α is corrected by a correlative voluntary movement β , so that we are brought back to the totality A .

Suppose now that another external change α' makes us pass anew from the totality A to the totality B .

Experience teaches us that this change α' is, like α , susceptible of being corrected by a correlative voluntary movement β' and that this movement β' corresponds to the same muscular sensations as the movement β which corrected α .

This fact is usually enunciated by saying that *space is homogeneous and isotropic*.

It may also be said that a movement which has once been produced may be repeated a second and a third time, and so on, without its properties varying.

In the first chapter, where we discussed the nature of mathematical reasoning, we saw the importance which must be attributed to the possibility of repeating indefinitely the same operation.

It is from this repetition that mathematical reasoning gets its power; it is, therefore, thanks to the law of homogeneity, that it has a hold on the geometric facts.

For completeness, to the law of homogeneity should be added a multitude of other analogous laws, into the details of which I do not wish to enter, but which mathematicians sum up in a word by saying that displacements form 'a group.'

THE NON-EUCLIDEAN WORLD.—If geometric space were a frame imposed on *each* of our representations, considered individually, it would be impossible to represent to ourselves an image stripped of this frame, and we could change nothing of our geometry.

But this is not the case; geometry is only the résumé of the laws according to which these images succeed each other. Nothing then prevents us from imagining a series of representations, similar in all points to our ordinary representations but succeeding one another according to laws different from those to which we are accustomed.

We can conceive then that beings who received their education in an environment where these laws were thus upset might have a geometry very different from ours.

Suppose, for example, a world enclosed in a great sphere and subject to the following laws:

The temperature is not uniform; it is greatest at the center, and diminishes in proportion to the distance from the center, to sink to absolute zero when the sphere is reached in which this world is enclosed.

To specify still more precisely the law in accordance with which this temperature varies: Let R be the radius of the limiting sphere; let r be the distance of the point considered from the center of this sphere. The absolute temperature shall be proportional to $R^2 - r^2$.

I shall further suppose that, in this world, all bodies have the same coefficient of dilatation, so that the length of any rule is proportional to its absolute temperature.

Finally, I shall suppose that a body transported from one point to another of different temperature is put immediately into thermal equilibrium with its new environment.

Nothing in these hypotheses is contradictory or unimaginable.

A movable object will then become smaller and smaller in proportion as it approaches the limit-sphere.

Note first that, though this world is limited from the point of view of our ordinary geometry, it will appear infinite to its inhabitants.

In fact, when these try to approach the limit-sphere, they cool off and become smaller and smaller. Therefore the steps they take are also smaller and smaller, so that they can never reach the limiting sphere.

If, for us, geometry is only the study of the laws according to which rigid solids move, for these imaginary beings it will be the study of the laws of motion of solids *distorted by the differences of temperature* just spoken of.

No doubt, in our world, natural solids likewise undergo variations of form and volume due to warming or cooling. But we neglect these variations in laying the foundations of geometry, because, besides their being very slight, they are irregular and consequently seem to us accidental.

In our hypothetical world, this would no longer be the case, and these variations would follow regular and very simple laws.

Moreover, the various solid pieces of which the bodies of its inhabitants would be composed would undergo the same variations of form and volume.

I will make still another hypothesis; I will suppose light traverses media diversely refractive and such that the index of refraction is inversely proportional to $R^2 - r^2$. It is easy to see that, under these conditions, the rays of light would not be rectilinear, but circular.

To justify what precedes, it remains for me to show that certain changes in the position of external objects can be *corrected* by correlative movements of the sentient beings inhabiting this

imaginary world, and that in such a way as to restore the primitive aggregate of impressions experienced by these sentient beings.

Suppose in fact that an object is displaced, undergoing deformation, not as a rigid solid, but as a solid subjected to unequal dilatations in exact conformity to the law of temperature above supposed. Permit me for brevity to call such a movement a *non-Euclidean displacement*.

If a sentient being happens to be in the neighborhood, his impressions will be modified by the displacement of the object, but he can reestablish them by moving in a suitable manner. It suffices if finally the aggregate of the object and the sentient being, considered as forming a single body, has undergone one of those particular displacements I have just called non-Euclidean. This is possible if it be supposed that the limbs of these beings dilate according to the same law as the other bodies of the world they inhabit.

Although from the point of view of our ordinary geometry there is a deformation of the bodies in this displacement and their various parts are no longer in the same relative position, nevertheless we shall see that the impressions of the sentient being have once more become the same.

In fact, though the mutual distances of the various parts may have varied, yet the parts originally in contact are again in contact. Therefore the tactile impressions have not changed.

On the other hand, taking into account the hypothesis made above in regard to the refraction and the curvature of the rays of light, the visual impressions will also have remained the same.

These imaginary beings will therefore like ourselves be led to classify the phenomena they witness and to distinguish among them the 'changes of position' susceptible of correction by a correlative voluntary movement.

If they construct a geometry, it will not be, as ours is, the study of the movements of our rigid solids; it will be the study of the changes of position which they will thus have distinguished and which are none other than the 'non-Euclidean displacements'; *it will be non-Euclidean geometry*.

Thus beings like ourselves, educated in such a world, would not have the same geometry as ours.

THE WORLD OF FOUR DIMENSIONS.—We can represent to ourselves a four-dimensional world just as well as a non-Euclidean.

The sense of sight, even with a single eye, together with the muscular sensations relative to the movements of the eyeball, would suffice to teach us space of three dimensions.

The images of external objects are painted on the retina, which is a two-dimensional canvas; they are *perspectives*.

But, as eye and objects are movable, we see in succession various perspectives of the same body, taken from different points of view.

At the same time, we find that the transition from one perspective to another is often accompanied by muscular sensations.

If the transition from the perspective A to the perspective B , and that from the perspective A' to the perspective B' are accompanied by the same muscular sensations, we liken them one to the other as operations of the same nature.

Studying then the laws according to which these operations combine, we recognize that they form a group, which has the same structure as that of the movements of rigid solids.

Now, we have seen that it is from the properties of this group we have derived the notion of geometric space and that of three dimensions.

We understand thus how the idea of a space of three dimensions could take birth from the pageant of these perspectives, though each of them is of only two dimensions, since *they follow one another according to certain laws*.

Well, just as the perspective of a three-dimensional figure can be made on a plane, we can make that of a four-dimensional figure on a picture of three (or of two) dimensions. To a geometer this is only child's play.

We can even take of the same figure several perspectives from several different points of view.

We can easily represent to ourselves these perspectives, since they are of only three dimensions.

Imagine that the various perspectives of the same object succeed one another, and that the transition from one to the other is accompanied by muscular sensations.

We shall of course consider two of these transitions as two

operations of the same nature when they are associated with the same muscular sensations.

Nothing then prevents us from imagining that these operations combine according to any law we choose, for example, so as to form a group with the same structure as that of the movements of a rigid solid of four dimensions.

Here there is nothing unpicturable, and yet these sensations are precisely those which would be felt by a being possessed of a two-dimensional retina who could move in space of four dimensions. In this sense we may say the fourth dimension is imaginable.

CONCLUSIONS.—We see that experience plays an indispensable rôle in the genesis of geometry; but it would be an error thence to conclude that geometry is, even in part, an experimental science.

If it were experimental, it would be only approximative and provisional. And what rough approximation!

Geometry would be only the study of the movements of solids; but in reality it is not occupied with natural solids, it has for object certain ideal solids, absolutely rigid, which are only a simplified and very remote image of natural solids.

The notion of these ideal solids is drawn from all parts of our mind, and experience is only an occasion which induces us to bring it forth from them.

The object of geometry is the study of a particular 'group'; but the general group concept pre-exists, at least potentially, in our minds. It is imposed on us, not as form of our sense, but as form of our understanding.

Only, from among all the possible groups, that must be chosen which will be, so to speak, the *standard* to which we shall refer natural phenomena.

Experience guides us in this choice without forcing it upon us; it tells us not which is the truest geometry, but which is the most *convenient*.

Notice that I have been able to describe the fantastic worlds above imagined *without ceasing to employ the language of ordinary geometry*.

And, in fact, we should not have to change it if transported thither.

Beings educated there would doubtless find it more convenient to create a geometry different from ours, and better adapted to their impressions. As for us, in face of the *same* impressions, it is certain we should find it more convenient not to change our habits.

CHAPTER V.

EXPERIENCE AND GEOMETRY.

1. ALREADY in the preceding pages I have several times tried to show that the principles of geometry are not experimental facts and that in particular Euclid's postulate can not be proven experimentally.

However decisive appear to me the reasons already given, I believe I should emphasize this point because here a false idea is profoundly rooted in many minds.

2. If we construct a material circle, measure its radius and circumference, and see if the ratio of these two lengths is equal to π , what shall we have done? We shall have made an experiment on the properties of the matter with which we constructed this *round thing*, and of that of which the measure used was made.

3. GEOMETRY AND ASTRONOMY.—The question has also been put in another way. If Lobachevski's geometry is true, the parallax of a very distant star will be finite; if Riemann's is true, it will be negative. These are results which seem within the reach of experiment, and there have been hopes that astronomical observations might enable us to decide between the three geometries.

But in astronomy 'straight line' means simply 'path of a ray of light.'

If therefore negative parallaxes were found, or if it were demonstrated that all parallaxes are superior to a certain limit, two courses would be open to us; we might either renounce Euclidean geometry, or else modify the laws of optics and suppose that light does not travel rigorously in a straight line.

It is needless to add that all the world would regard the latter solution as the more advantageous.

The Euclidean geometry has therefore nothing to fear from fresh experiments.

4. Is the position tenable, that certain phenomena, possible in Euclidean space, would be impossible in non-Euclidean space, so that experience, in establishing these phenomena, would di-

rectly contradict the non-Euclidean hypothesis? For my part I think no such question can be put. To my mind it is precisely equivalent to the following, whose absurdity is patent to all eyes: are there lengths expressible in meters and centimeters, but which can not be measured in fathoms, feet and inches, so that experience, in ascertaining the existence of these lengths, would directly contradict the hypothesis that there are fathoms divided into six feet?

Examine the question more closely. I suppose that the straight line possesses in Euclidean space any two properties which I shall call *A* and *B*; that in non-Euclidean space it still possesses the property *A*, but no longer has the property *B*; finally I suppose that in both Euclidean and non-Euclidean space, the straight line is the only line having the property *A*.

If this were so, experience would be capable of deciding between the hypothesis of Euclid and that of Lobachevski. It would be ascertained that a definite concrete object, accessible to experiment, for example, a pencil of rays of light, possesses the property *A*; we should conclude that it is rectilinear, and then investigate whether or not it has the property *B*.

But *this is not so*; no property exists which, like this property *A*, can be an absolute criterion enabling us to recognize the straight line and to distinguish it from every other line.

Shall we say, for instance: "the following is such a property: the straight line is a line such that a figure of which this line forms a part can be moved without the mutual distances of its points varying and so that all points of this line remain fixed"?

This, in fact, is a property which, in Euclidean or non-Euclidean space, belongs to the straight and belongs only to it. But how shall we ascertain experimentally whether it belongs to this or that concrete object? It will be necessary to measure distances, and how shall one know that any concrete magnitude which I have measured with my material instrument really represents the abstract distance?

We have only pushed back the difficulty.

In reality the property just enunciated is not a property of the straight line alone, it is a property of the straight line and distance. For it to serve as absolute criterion, we should have

to be able to establish not only that it does not also belong to a line other than the straight and to distance, but in addition that it does not belong to a line other than the straight and to a magnitude other than distance. Now this is not true.

It is therefore impossible to imagine a concrete experiment which can be interpreted in the Euclidean system and not in the Lobachevskian system, so that I may conclude:

No experience will ever be in contradiction to Euclid's postulate; nor, on the other hand, will any experience ever contradict the postulate of Lobachevski.

5. But it is not enough that the Euclidean (or non-Euclidean) geometry can never be directly contradicted by experience. Might it not happen that it can accord with experience only by violating the principle of sufficient reason or that of the relativity of space?

I will explain myself: consider any material system; we shall have to regard, on the one hand, 'the state' of the various bodies of this system (for instance, their temperature, their electric potential, etc.), and, on the other hand, their position in space; and among the data which enable us to define this position, we shall, moreover, distinguish the mutual distances of these bodies, which define their relative positions, from the conditions which define the absolute position of the system and its absolute orientation in space.

The laws of the phenomena which will happen in this system will depend on the state of these bodies and their mutual distances; but, because of the relativity and passivity of space, they will not depend on the absolute position and orientation of the system.

In other words, the state of the bodies and their mutual distances at any instant will depend solely on the state of these same bodies and on their mutual distances at the initial instant, but will not at all depend on the absolute initial position of the system or on its absolute initial orientation. This is what for brevity I shall call *the law of relativity*.

Hitherto I have spoken as a Euclidean geometer. As I have said, an experience, whatever it be, admits of an interpretation on the Euclidean hypothesis; but it admits of one equally on the non-Euclidean hypothesis. Well, we have made a series of

experiments; we have interpreted them on the Euclidean hypothesis, and we have recognized that these experiments thus interpreted do not violate this 'law of relativity.'

We now interpret them on the non-Euclidean hypothesis: this is always possible; only the non-Euclidean distances of our different bodies in this new interpretation will not generally be the same as the Euclidean distances in the primitive interpretation.

Will our experiments, interpreted in this new manner, still be in accord with our 'law of relativity'? And if there were not this accord, should we not have also the right to say experience had proven the falsity of the non-Euclidean geometry?

It is easy to see that this is an idle fear; in fact, to apply the law of relativity in all rigor, it must be applied to the entire universe. For if only a part of this universe were considered, and if the absolute position of this part happened to vary, the distances to the other bodies of the universe would likewise vary, their influence on the part of the universe considered would consequently augment or diminish, which might modify the laws of the phenomena happening there.

But if our system is the entire universe, experience is powerless to give information about its absolute position and orientation in space. All that our instruments, however perfected they may be, can tell us will be the state of the various parts of the universe and their mutual distances.

So our law of relativity may be thus enunciated:

The readings we shall be able to make on our instruments at any instant, will depend only on the readings we could have made on these same instruments at the initial instant.

Now such an enunciation is independent of every interpretation of experimental facts. If the law is true in the Euclidean interpretation, it will also be true in the non-Euclidean interpretation.

Allow me here a short digression. I have spoken above of the data which define the position of the various bodies of the system; I should likewise have spoken of those which define their velocities; I should then have had to distinguish the velocities with which the mutual distances of the different bodies vary; and, on the other hand, the velocities of translation and rotation

of the system, that is to say, the velocities with which its absolute position and orientation vary.

To fully satisfy the mind, the law of relativity should be expressible thus:

The state of bodies and their mutual distances at any instant, as well as the velocities with which these distances vary at this same instant, will depend only on the state of those bodies and their mutual distances at the initial instant, and the velocities with which these distances vary at this initial instant, but they will not depend either upon the absolute initial position of the system, or upon its absolute orientation, or upon the velocities with which this absolute position and orientation varied at the initial instant.

Unhappily the law thus enunciated is not in accord with experiments, at least as they are ordinarily interpreted.

Suppose a man be transported to a planet whose heavens were always covered with a thick curtain of clouds, so that he could never see the other stars; on that planet he would live as if it were isolated in space. Yet this man could become aware that it turned, either by measuring its oblateness (done ordinarily by the aid of astronomic observations, but capable of being done by purely geodetic means), or by repeating the experiment of Foucault's pendulum. The absolute rotation of this planet could therefore be made evident.

That is a fact which shocks the philosopher, but which the physicist is compelled to accept.

We know that from this fact Newton inferred the existence of absolute space; I myself am quite unable to adopt this view. I shall explain why in Part Third. For the moment it is not my intention to enter upon this difficulty.

Therefore I must resign myself, in the enunciation of the law of relativity, to including velocities of every kind among the data which define the state of the bodies.

However that may be, this difficulty is the same for Euclid's geometry as for Lobachevski's; I therefore need not trouble myself with it, and have only mentioned it incidentally.

What is important is the conclusion: experiment can not decide between Euclid and Lobachevski.

To sum up, whichever way we look at it, it is impossible to discover in geometric empiricism a rational meaning.

6. Experiments only teach us the relations of bodies to one another; none of them bears or can bear on the relations of bodies with space, or on the mutual relations of different parts of space.

"Yes," you reply, "a single experiment is insufficient, because it gives me only a single equation with several unknowns; but when I shall have made enough experiments I shall have equations enough to calculate all my unknowns."

To know the height of the main-mast does not suffice for calculating the age of the captain. When you have measured every bit of wood in the ship you will have many equations, but you will know his age no better. All your measurements bearing only on your bits of wood can reveal to you nothing except concerning these bits of wood. Just so your experiments, however numerous they may be, bearing only on the relations of bodies to one another, will reveal to us nothing about the mutual relations of the various parts of space.

7. Will you say that if the experiments bear on the bodies, they bear at least upon the geometric properties of the bodies? But, first, what do you understand by geometric properties of the bodies? I assume that it is a question of the relations of the bodies with space; these properties are therefore inaccessible to experiments which bear only on the relations of the bodies to one another. This alone would suffice to show that there can be no question of these properties.

Still let us begin by coming to an understanding about the sense of the phrase: geometric properties of bodies. When I say a body is composed of several parts, I assume that I do not enunciate therein a geometric property, and this would remain true even if I agreed to give the improper name of points to the smallest parts I consider.

When I say that such a part of such a body is in contact with such a part of such another body, I enunciate a proposition which concerns the mutual relations of these two bodies and not their relations with space.

I suppose you will grant me these are not geometric properties; at least I am sure you will grant me these properties are independent of all knowledge of metric geometry.

This presupposed, I imagine that we have a solid body formed of eight slender iron rods, $OA, OB, OC, OD, OE, OF, OG, OH$, united at one of their extremities O . Let us besides have a second solid body, for example a bit of wood, to be marked with three little flecks of ink which I shall call α, β, γ . I further suppose it ascertained that $\alpha\beta\gamma$ may be brought into contact with AGO (I mean α with A , and at the same time β with G and γ with O), then that we may bring successively into contact $\alpha\beta\gamma$ with BGO, CGO, DGO, EGO, FGO , then with $AHO, BHO, CHO, DHO, EHO, FHO$, then $\alpha\gamma$ successively with AB, BC, CD, DE, EF, FA .

These are determinations we may make without having in advance any notion about form or about the metric properties of space. They in no wise bear on the 'geometric properties of bodies.' And these determinations will not be possible if the bodies experimented upon move in accordance with a group having the same structure as the Lobachevskian group (I mean according to the same laws as solid bodies in Lobachevski's geometry). They suffice therefore to prove that these bodies move in accordance with the Euclidean group, or at least that they do not move according to the Lobachevskian group.

That they are compatible with the Euclidean group is easy to see. For they could be made if the body $\alpha\beta\gamma$ was a rigid solid of our ordinary geometry presenting the form of a right-angled triangle and if the points $ABCDEFGH$ were the summits of a polyhedron formed of two regular hexagonal pyramids of our ordinary geometry, having for common base $ABCDEF$ and for apices the one G and the other H .

Suppose now that in place of the preceding determinations it is observed that as above $\alpha\beta\gamma$ can be successively applied to $AGO, BGO, CGO, DGO, EGO, AHO, BHO, CHO, DHO, EHO, FHO$, then that $\alpha\beta$ (and no longer $\alpha\gamma$) can be successively applied to AB, BC, CD, DE, EF , and FA .

These are determinations which could be made if non-Euclidean geometry were true, if the bodies $\alpha\beta\gamma$ and $OABCDEFGH$ were rigid solids, and if the first were a right-angled triangle and the second a double regular hexagonal pyramid of suitable dimensions.

Therefore these new determinations are not possible if the

bodies move according to the Euclidean group; but they become so if it be supposed that the bodies move according to the Lobachevskian group. They would suffice therefore (if one made them) to prove that the bodies in question do not move according to the Euclidean group.

Thus, without making any hypothesis about form, about the nature of space, about the relations of bodies to space, and without attributing to bodies any geometric property, I have made observations which have enabled me to show in one case that the bodies experimented upon move according to a group whose structure is Euclidean, in the other case that they move according to a group whose structure is Lobachevskian.

And one may not say that the first aggregate of determinations would constitute an experiment proving that space is Euclidean, and the second an experiment proving that space is non-Euclidean.

In fact one could imagine (I say imagine) bodies moving so as to render possible the second series of determinations. And the proof is that the first mechanician met could construct such bodies if he cared to take the pains and make the outlay. You will not conclude from that, however, that space is non-Euclidean.

Nay, since the ordinary solid bodies would continue to exist when the mechanician had constructed the strange bodies of which I have just spoken, it would be necessary to conclude that space is at the same time Euclidean and non-Euclidean.

Suppose, for example, that we have a great sphere of radius R and that the temperature decreases from the center to the surface of this sphere according to the law of which I have spoken in describing the non-Euclidean world.

We might have bodies whose expansion would be negligible and which would act like ordinary rigid solids; and, on the other hand, bodies very dilatable and which would act like non-Euclidean solids. We might have two double pyramids $OABCDEFGH$ and $O'A'B'C'D'E'F'G'H'$ and two triangles $a\beta\gamma$ and $a'\beta'\gamma'$. The first double pyramid might be rectilinear and the second curvilinear; the triangle $a\beta\gamma$ might be made of inexpandible matter and the other of a very dilatable matter.

It would then be possible to make the first observations with the double pyramid OAH and the triangle $a\beta\gamma$, and the second with

the double pyramid $O'A'H'$ and the triangle $\alpha'\beta'\gamma'$. And then experiment would seem to prove first that the Euclidean geometry is true and then that it is false.

Experiments therefore have a bearing, not on space, but on bodies.

SUPPLEMENT.

8. To complete the matter, I ought to speak of a very delicate question, which would require long developments; I shall confine myself to summarizing here what I have expounded in the *Revue de Métaphysique et de Morale* and in *The Monist*. When we say space has three dimensions, what do we mean?

We have seen the importance of those 'internal changes' revealed to us by our muscular sensations. They may serve to characterize the various *attitudes* of our body. Take arbitrarily as origin one of these attitudes A . When we pass from this initial attitude to any other attitude B , we feel a series of muscular sensations, and this series S will define B . Observe, however, that we shall often regard two series S and S' as defining the same attitude B (since the initial and final attitudes A and B remaining the same, the intermediary attitudes and the corresponding sensations may differ). How then shall we recognize the equivalence of these two series? Because they may serve to compensate the same external change, or more generally because, when it is a question of compensating an external change, one of the series can be replaced by the other. Among these series, we have distinguished those which of themselves alone can compensate an external change, and which we have called 'displacements.' As we can not discriminate between two displacements which are too close together, the totality of these displacements presents the characteristics of a physical continuum; experience teaches us that they are those of a physical continuum of six dimensions; but as we do not yet know how many dimensions space itself has, as we must first solve another question.

What is a point of space? Everybody thinks he knows, but that is an illusion. What we see when we try to represent to ourselves a point of space is a black speck on white paper, a speck of chalk on a blackboard, always an object. The question should therefore be understood as follows:

What do I mean when I say the object *B* is at the same point that the object *A* occupied just now? Or further, what criterion will enable me to apprehend this?

I mean that, *although I have not budged* (which my muscular sense tells me), my first finger which just now touched the object *A* touches at present the object *B*. I could have used other criteria, for instance another finger or the sense of sight. But the first criterion is sufficient; I know that if it answers yes, all the other criteria will give the same response. I know it *by experience*, I can not know it *a priori*. For the same reason I say that touch can not be exercised at a distance; this is another way of enunciating the same experimental fact. And if, on the contrary, I say that sight acts at a distance, it means that the criterion furnished by sight may respond yes while the others reply no.

And in fact, the object, although moved away, may form its image at the same point of the retina. Sight responds yes, the object has remained at the same point, and touch answers no, because my finger which just now touched the object, at present touches it no longer. If experience has shown us that one finger may respond no when the other says yes, we should likewise say that touch acts at a distance.

In short, for each attitude of my body, my first finger determines a point and this it is, and this alone, which defines a point of space.

To each attitude corresponds thus a point; but it often happens that the same point corresponds to several different attitudes (in this case we say our finger has not budged, but the rest of the body has moved). We distinguish therefore among the changes of attitude those where the finger does not budge. How are we led thereto? It is because often we notice that in these changes the object which is in contact with the finger remains in contact with it.

Range therefore in the same class all the attitudes obtainable from each other by one of the changes we have thus distinguished. To all the attitudes of the class will correspond the same point of space. Therefore to each class will correspond a point and to each point a class. But one may say that what experience arrives at is not the point, it is this class of changes, or better, the corresponding class of muscular sensations.

And when we say space has three dimensions, we simply mean that the totality of these classes appears to us with the characteristics of a physical continuum of three dimensions.

One might be tempted to conclude that it is experience which has taught us how many dimensions space has. But in reality here also our experiences have bearing, not on space, but on our body and its relations with the neighboring objects. Moreover they are excessively crude.

In our mind pre-existed the latent idea of a certain number of groups—those whose theory Lie has developed. Which group shall we choose, to make of it a sort of standard with which to compare natural phenomena? And, this group chosen, which of its sub-groups shall we take to characterize a point of space? Experience has guided us by showing us which choice best adapts itself to the properties of our body. But its rôle is limited to that.

ANCESTRAL EXPERIENCE.

It has often been said that if individual experience could not create geometry, the same is not true of ancestral experience. But what does that mean? Is it meant that we could not experimentally demonstrate Euclid's postulate, but that our ancestors have been able to do it? Not in the least. It is meant that by natural selection our mind has *adapted* itself to the conditions of the external world, that it has adopted the geometry *most advantageous* to the species; or in other words *the most convenient*. This is entirely in conformity with our conclusions; geometry is not true, it is advantageous.

PART III.

FORCE.

CHAPTER VI.

THE CLASSIC MECHANICS.

THE English teach mechanics as an experimental science; on the continent it is always expounded as more or less a deductive and *a priori* science. The English are right, that goes without saying; but how could the other method have been persisted in so long? Why have the continental savants who have sought to get out of the ruts of their predecessors been usually unable to free themselves completely?

On the other hand, if the principles of mechanics are only of experimental origin, are they not therefore only approximate and provisional? Might not new experiments some day lead us to modify or even to abandon them?

Such are the questions which naturally obtrude themselves, and the difficulty of solution comes principally from the fact that the treatises on mechanics do not clearly distinguish between what is experiment, what is mathematical reasoning, what is convention, what is hypothesis.

That is not all:

1° There is no absolute space and we can conceive only of relative motions; yet usually the mechanical facts are enunciated as if there were an absolute space to which to refer them.

2° There is no absolute time; to say two durations are equal is an assertion which has by itself no meaning and which can acquire one only by convention.

3° Not only have we no direct intuition of the equality of two durations, but we have not even direct intuition of the

simultaneity of two events occurring in different places: this I have explained in an article entitled *La mesure du temps*.*

4° Finally, our Euclidean geometry is itself only a sort of convention of language; mechanical facts might be enunciated with reference to a non-Euclidean space which would be a guide less convenient than, but just as legitimate as our ordinary space; the enunciation would thus become much more complicated, but it would remain possible.

Thus absolute space, absolute time, geometry itself, are not conditions which impose themselves on mechanics; all these things are no more antecedent to mechanics than the French language is logically antecedent to the verities one expresses in French.

We might try to enunciate the fundamental laws of mechanics in a language independent of all these conventions; we should thus without doubt get a better idea of what these laws are in themselves; this is what M. Andrade has attempted to do, at least in part, in his *Leçons de mécanique physique*.

The enunciation of these laws would become of course much more complicated, because all these conventions have been devised expressly to abridge and simplify this enunciation.

As for me, save in what concerns absolute space, I shall ignore all these difficulties; not that I fail to appreciate them, far from that; but we have sufficiently examined them in the first two parts of the book.

I shall therefore admit, *provisionally*, absolute time and Euclidean geometry.

THE PRINCIPLE OF INERTIA.—A body acted on by no force can only move uniformly in a straight line.

Is this a truth imposed *a priori* upon the mind? If it were so, how should the Greeks have failed to recognize it? How could they have believed that motion stops when the cause which gave birth to it ceases? Or again that every body, if nothing prevents, will move in a circle, the noblest of motions?

If it is said that the velocity of a body can not change if there is no reason for it to change, could it not be maintained just as well that the position of this body can not change, or that the curvature of its trajectory can not change, if no external cause intervenes to modify them?

* *Revue de Métaphysique et de Morale*, t. VI., pp. 1-13 (January, 1898).

Is the principle of inertia, which is not an *a priori* truth, therefore an experimental fact? But has any one ever experimented on bodies withdrawn from the action of every force? and, if so, how was it known that these bodies were subjected to no force? The example ordinarily cited is that of a ball rolling a very long time on a marble table; but why do we say it is subjected to no force? Is this because it is too remote from all other bodies to experience any appreciable action from them? Yet it is not farther from the earth than if it were thrown freely into the air; and every one knows that in this case it would experience the influence of gravity due to the attraction of the earth.

Teachers of mechanics usually pass rapidly over the example of the ball; but they add that the principle of inertia is verified indirectly by its consequences. They express themselves badly; they evidently mean it is possible to verify various consequences of a more general principle, of which that of inertia is only a particular case.

I shall propose for this general principle the following enunciation:

The acceleration of a body depends only upon the position of this body and of the neighboring bodies and upon their velocities.

Mathematicians would say the movements of all the material molecules of the universe depend on differential equations of the second order.

To make it clear that this is really the natural generalization of the law of inertia, I shall beg you to permit me a bit of fiction. The law of inertia, as I have said above, is not imposed upon us *a priori*; other laws would be quite as compatible with the principle of sufficient reason. If a body is subjected to no force, in lieu of supposing its velocity not to change, it might be supposed that it is its position or else its acceleration which is not to change.

Well, imagine for an instant that one of these two hypothetical laws is a law of nature and replaces our law of inertia. What would be its natural generalization? A moment's thought will show us.

In the first case, we must suppose that the velocity of a body depends only upon its position and upon that of the neigh-

boring bodies; in the second case, that the change of acceleration of a body depends only upon the position of this body and of the neighboring bodies, upon their velocities and upon their accelerations.

Or to speak the language of mathematics, the differential equations of motion would be of the first order in the first case, and of the third order in the second case.

Let us slightly modify our fiction. Suppose a world analogous to our solar system, but where, by a strange chance, the orbits of all the planets are without eccentricity and without inclination. Suppose further that the masses of these planets are too slight for their mutual perturbations to be sensible. Astronomers inhabiting one of these planets could not fail to conclude that the orbit of a star can only be circular and parallel to a certain plane; the position of a star at a given instant would then suffice to determine its velocity and its whole path. The law of inertia which they would adopt would be the first of the two hypothetical laws I have mentioned.

Imagine now that this system is some day traversed with great velocity by a body of vast mass, coming from distant constellations. All the orbits would be profoundly disturbed. Still our astronomers would not be too greatly astonished; they would very well divine that this new star was alone to blame for all the mischief. "But," they would say, "when it is gone, order will of itself be reestablished; no doubt the distances of the planets from the sun will not revert to what they were before the cataclysm, but when the perturbing star is gone, the orbits will again become circular."

It would only be when the disturbing body was gone and when nevertheless the orbits, in lieu of again becoming circular, became elliptic, that these astronomers would become conscious of their error and the necessity of re-making all their mechanics.

I have dwelt somewhat upon these hypotheses, because it seems to me one can clearly comprehend what our generalized law of inertia really is only in contrasting it with a contrary hypothesis.

Well, now, has this generalized law of inertia been verified by experiment, or can it be? When Newton wrote the *Principia*

he quite regarded this truth as experimentally acquired and demonstrated. It was so in his eyes, not only through the anthropomorphism of which we shall speak further on, but through the work of Galileo. It was so even from Kepler's laws themselves; in accordance with these laws, in fact, the path of a planet is completely determined by its initial position and initial velocity; this is just what our generalized law of inertia requires.

For this principle to be only in appearance true, for one to have cause to dread having some day to replace it by one of the analogous principles I have just now contrasted with it, would be necessary our having been misled by some amazing chance, like that which, in the fiction above developed, led into error our imaginary astronomers.

Such an hypothesis is too unlikely to delay over. No one will believe that such coincidences can happen; no doubt the probability of two eccentricities being both precisely null, to within errors of observation, is not less than the probability of one being precisely equal to 0.1, for instance, and the other to 0.2, to within errors of observation. The probability of a simple event is not less than that of a complicated event; and yet, if the first happens, we shall not consent to attribute it to chance; we should not believe that nature had acted expressly to deceive us. The hypothesis of an error of this sort being discarded, it may therefore be admitted that in so far as astronomy is concerned, our law has been verified by experiment.

But astronomy is not the whole of physics.

May we not fear lest some day a new experiment should come to falsify the law in some domain of physics? An experimental law is always subject to revision; one should always expect to see it replaced by a more precise law.

Yet no one seriously thinks that the law we are speaking of will ever be abandoned or amended. Why? Precisely because it can never be subjected to a decisive test.

First of all, in order that this trial should be complete, it would be necessary that after a certain time all the bodies in the universe should revert to their initial positions with their initial velocities. It might then be seen whether, starting from this moment, they would resume their original paths.

But this test is impossible, it can be only partially applied, and, however well it is made, there will always be some bodies which will not revert to their initial positions; thus every derogation of the law will easily find its explanation.

This is not all; in astronomy we *see* the bodies whose motions we study, and we usually assume that they are not subjected to the action of other invisible bodies. Under these conditions our law must indeed be either verified or not verified.

But it is not the same in physics; if the physical phenomena are due to motions, it is to the motions of molecules which we do not see. If then the acceleration of one of the bodies we see appears to us to depend on *something else* besides the positions or velocities of other visible bodies or of invisible molecules whose existence we have been previously led to admit, nothing prevents our supposing that this *something else* is the position or the velocity of other molecules whose presence we have not before suspected. The law will find itself safeguarded.

Permit me to employ mathematical language a moment to express the same thought under another form. Suppose we observe n molecules and ascertain that their $3n$ coordinates satisfy a system of $3n$ differential equations of the fourth order (and not of the second order as the law of inertia would require). We know that by introducing $3n$ auxiliary variables, a system of $3n$ equations of the fourth order can be reduced to a system of $6n$ equations of the second order. If then we suppose these $3n$ auxiliary variables represent the coordinates of n visible molecules, the result is again in conformity with the law of inertia.

To sum up, this law, verified experimentally in some particular cases, may unhesitatingly be extended to the most general cases, since we know that in these general cases experiment no longer is able either to confirm or to contradict it.

THE LAW OF ACCELERATION.—The acceleration of a body is equal to the force acting on it divided by its mass. Can this law be verified by experiment? For that, it would be necessary to measure the three magnitudes which figure in the enunciation: acceleration, force and mass.

I assume that acceleration can be measured, for I pass over the difficulty arising from the measurement of time. But how measure force, or mass? We do not even know what they are.

What is *mass*? According to Newton, it is the product of the volume by the density. According to Thomson and Tait, it would be better to say that density is the quotient of the mass by the volume. What is *force*? It is, replies Lagrange, that which moves or tends to move a body. It is, Kirchhoff will say, the product of the mass by the *acceleration*. But then, why not say the mass is the quotient of the force by the acceleration?

These difficulties are inextricable.

When we say force is the cause of motion, we talk metaphysics, and this definition, if one were content with it, would be absolutely sterile. For a definition to be of any use, it must teach us to *measure* force; moreover that suffices; it is not at all necessary that it teach us what force is *in itself*, nor whether it is the cause or the effect of motion.

We must therefore first define the equality of two forces. When shall we say two forces are equal? It is, we are told, when, applied to the same mass, they impress upon it the same acceleration, or when, opposed directly one to the other, they produce equilibrium. This definition is only a sham. A force applied to a body can not be uncoupled to hook it up to another body, as one uncouples a locomotive to attach it to another train. It is therefore impossible to know what acceleration such a force, applied to such a body, would impress upon such an other body, *if* it were applied to it. It is impossible to know how two forces which are not directly opposed would act, *if* they were directly opposed.

It is this definition we try to materialize, so to speak, when we measure a force with a dynamometer, or in balancing it with a weight. Two forces F and F' , which for simplicity I will suppose vertical and directed upward, are applied respectively to two bodies C and C' ; I suspend the same heavy body P first to the body C , then to the body C' ; if equilibrium is produced in both cases, I shall conclude that the two forces F and F' are equal to one another, since they are each equal to the weight of the body P .

But am I sure the body P has retained the same weight when I have transported it from the first body to the second? Far from it; *I am sure of the contrary*; I know the intensity of gravity varies from one point to another, and that it is stronger, for in-

stance, at the pole than at the equator. No doubt the difference is very slight and, in practise, I shall take no account of it; but a properly constructed definition should have a mathematical rigor; this rigor is lacking. What I say of weight would evidently apply to the force of the resiliency of a dynamometer, which the temperature and a multitude of circumstances may cause to vary.

This is not all; we can not say the weight of the body P may be applied to the body C and directly balance the force F . What is applied to the body C is the action A of the body P on the body C ; the body P is submitted on its part, on the one hand, to its weight; on the other hand, to the reaction R of the body C on P . Finally, the force F is equal to the force A , since it balances it; the force A is equal to R , in virtue of the principle of the equality of action and reaction; lastly, the force R is equal to the weight of P , since it balances it. It is from these three equalities we deduce as consequence the equality of F and the weight of P .

We are therefore obliged in the definition of the equality of the two forces to bring in the principle of the equality of action and reaction; *on this account, this principle must no longer be regarded as an experimental law, but as a definition.*

For recognizing the equality of two forces here, we are then in possession of two rules: equality of two forces which balance; equality of action and reaction. But, as we have seen above, these two rules are insufficient; we are obliged to have recourse to a third rule and to assume that certain forces, as, for instance, the weight of a body, are constant in magnitude and direction. But this third rule, as I have said, is an experimental law; it is only approximately true; *it is a bad definition.*

We are therefore reduced to Kirchhoff's definition; *force is equal to the mass multiplied by the acceleration.* This 'law of Newton' in its turn ceases to be regarded as an experimental law, it is now only a definition. But this definition is still insufficient, for we do not know what mass is. It enables us doubtless to calculate the relation of two forces applied to the same body at different instants; it teaches us nothing about the relation of two forces applied to two different bodies.

To complete it, it is necessary to go back anew to Newton's third law (equality of action and reaction), regarded again, not

as an experimental law, but as a definition. Two bodies A and B act one upon the other; the acceleration of A multiplied by the mass of A is equal to the action of B upon A ; in the same way, the product of the acceleration of B by its mass is equal to the reaction of A upon B . As, by definition, action is equal to reaction, the masses of A and B are in the inverse ratio of their accelerations. Here we have the ratio of these two masses defined, and it is for experiment to verify that this ratio is constant.

That would be all very well if the two bodies A and B alone were present and removed from the action of the rest of the world. This is not at all the case; the acceleration of A is not due merely to the action of B , but to that of a multitude of other bodies C, D, \dots . To apply the preceding rule, it is therefore necessary to separate the acceleration of A into many components, and discern which of these components is due to the action of B .

This separation would still be possible, if we *should assume* that the action of C upon A is simply adjoined to that of B upon A , without the presence of the body C modifying the action of B upon A ; or the presence of B modifying the action of C upon A ; if we should assume, consequently, that any two bodies attract each other, that their mutual action is along their join and depends only upon their distance apart; if, in a word, we assume *the hypothesis of central forces*.

You know that to determine the masses of the celestial bodies we use a wholly different principle. The law of gravitation teaches us that the attraction of two bodies is proportional to their masses; if r is their distance apart, m and m' their masses, k a constant, their attraction will be kmm'/r^2 .

What we are measuring then is not mass, the ratio of force to acceleration, but the attracting mass; it is not the inertia of the body, but its attracting force.

This is an indirect procedure, whose employment is not theoretically indispensable. It might very well have been that attraction was inversely proportional to the square of the distance without being proportional to the product of the masses, that it was equal to f/r^2 , but without our having $f = kmm'$.

If it were so, we could nevertheless, by observation of the *relative* motions of the heavenly bodies, measure the masses of these bodies.

But have we the right to admit the hypothesis of central forces? Is this hypothesis rigorously exact? Is it certain it will never be contradicted by experiment? Who would dare affirm that? And if we must abandon this hypothesis, the whole edifice so laboriously erected will crumble.

We have no longer the right to speak of the component of the acceleration of *A* due to the action of *B*. We have no means of distinguishing it from that due to the action of *C* or of another body. The rule for the measurement of masses becomes inapplicable.

What remains then of the principle of the equality of action and reaction? If the hypothesis of central forces is rejected, this principle should evidently be enunciated thus: the geometric resultant of all the forces applied to the various bodies of a system isolated from all external action will be null. Or, in other words, *the motion of the center of gravity of this system will be rectilinear and uniform.*

There it seems we have a means of defining mass; the position of the center of gravity evidently depends on the values attributed to the masses; it will be necessary to dispose of these values in such a way that the motion of the center of gravity may be rectilinear and uniform; this will always be possible if Newton's third law is true, and possible in general only in a single way.

But there exists no system isolated from all external action; all the parts of the universe are subject more or less to the action of all the other parts. *The law of the motion of the center of gravity is rigorously true only if applied to the entire universe.*

But then, to get from it the values of the masses, it would be necessary to observe the motion of the center of gravity of the universe. The absurdity of this consequence is manifest; we know only relative motions; the motion of the center of gravity of the universe will remain for us eternally unknown.

Therefore nothing remains and our efforts have been fruitless; we are driven to the following definition, which is only an avowal of powerlessness: *masses are coefficients it is convenient to introduce into calculations.*

We could reconstruct all mechanics by attributing different values to all the masses. This new mechanics would not be in

contradiction either with experience or with the general principles of dynamics (principle of inertia, proportionality of forces to masses and to accelerations, equality of action and reaction, rectilinear and uniform motion of the center of gravity, principle of areas).

Only the equations of this new mechanics would be *less simple*. Let us understand clearly: it would only be the first terms which would be less simple, that is those experience has already made us acquainted with; perhaps one could alter the masses by small quantities without the *complete* equations gaining or losing in simplicity.

Hertz has raised the question whether the principles of mechanics are rigorously true. "In the opinion of many physicists," he says, "it is inconceivable that the remotest experience should ever change anything in the immovable principles of mechanics; and yet, what comes from experience may always be rectified by experience." After what we have just said, these fears will appear groundless.

The principles of dynamics at first appeared to us as experimental truths; but we have been obliged to use them as definitions. It is *by definition* that force is equal to the product of mass by acceleration; here, then, is a principle which is henceforth beyond the reach of any further experiment. It is in the same way by definition that action is equal to reaction.

But then, it will be said, these unverifiable principles are absolutely devoid of any significance; experiment can not contradict them; but they can teach us nothing useful; then what is the use of studying dynamics?

This over-hasty condemnation would be unjust. There is not in nature any system *perfectly* isolated, perfectly removed from all external action; but there are systems *almost* isolated.

If such a system be observed, one may study not only the relative motion of its various parts one in reference to another, but also the motion of its center of gravity in reference to the other parts of the universe. We ascertain then that the motion of this center of gravity is *almost* rectilinear and uniform, in conformity with Newton's third law.

That is an experimental truth, but it can not be invalidated

by experience; in fact, what would a more precise experiment teach us? It would teach us that the law was only almost true; but that we knew already.

We can now understand how experience could have served as basis for the principles of mechanics and yet will never be able to contradict them.

ANTHROPOMORPHIC MECHANICS.—“Kirchhoff,” it will be said, “has only acted in obedience to the general tendency of mathematicians toward nominalism; from this his ability as a physicist has not saved him. He wanted a definition of force, and he took for it the first proposition that presented itself; but we need no definition of force: the idea of force is primitive, irreducible, indefinable; we know all that it is, we have a direct intuition of it. This direct intuition comes from the notion of effort, which is familiar to us from infancy.”

But first, even though this direct intuition made known to us the real nature of force in itself, it would be insufficient as a foundation for mechanics; it would besides be wholly useless. What is of importance is not to know what force is, but to know how to measure it.

Whatever does not teach us to measure it is as useless to mechanics as is, for instance, the subjective notion of warmth and cold to the physicist who is studying heat. This subjective notion can not be translated into numbers, therefore it is of no use; a scientist whose skin was an absolutely bad conductor of heat and who, consequently, would never have felt either sensations of cold or sensations of warmth, could read a thermometer just as well as any one else, and that would suffice him for constructing the whole theory of heat.

Now this immediate notion of effort is of no use to us for measuring force; it is clear, for instance, that I should feel more fatigue in lifting a weight of fifty kilos than a man accustomed to carry burdens.

But more than that: this notion of effort does not teach us the real nature of force; it reduces itself finally to a remembrance of muscular sensations, and it will hardly be maintained that the sun feels a muscular sensation when it draws the earth.

All that can there be sought is a symbol, less precise and less

convenient than the arrows the geometers use, but just as remote from the reality.

Anthropomorphism has played a considerable historic rôle in the genesis of mechanics; perhaps it will still at times furnish a symbol which will appear convenient to some minds; but it can not serve as foundation for anything of a truly scientific or philosophic character.

'THE SCHOOL OF THE THREAD.'—M. Andrade, in his *Leçons de mécanique physique*, has rejuvenated anthropomorphic mechanics. To the school of mechanics to which Kirchhoff belongs, he opposes that which he bizarrely calls the school of the thread.

This school tries to reduce everything to "the consideration of certain material systems of negligible mass, envisaged in the state of tension and capable of transmitting considerable efforts to distant bodies, systems of which the ideal type is the *thread*."

A thread which transmits any force is slightly elongated under the action of this force; the direction of the thread tells us the direction of the force, whose magnitude is measured by the elongation of the thread.

One may then conceive an experiment such as this. A body *A* is attached to a thread; at the other extremity of the thread any force acts which varies until the thread takes an elongation α ; the acceleration of the body *A* is noted; *A* is detached and the body *B* attached to the same thread; the same force or another force acts anew, and is made to vary until the thread takes again the elongation α ; the acceleration of the body *B* is noted. The experiment is then renewed with both *A* and *B*, but so that the thread takes the elongation β . The four observed accelerations should be proportional. We have thus an experimental verification of the law of acceleration above enunciated.

Or still better, a body is submitted to the simultaneous action of several identical threads in equal tension, and by experiment it is sought what must be the orientations of all these threads that the body may remain in equilibrium. We have then an experimental verification of the law of the composition of forces.

But, after all, what have we done? We have defined the force to which the thread is subjected by the deformation undergone by this thread, which is reasonable enough; we have further

assumed that if a body is attached to this thread, the effort transmitted to it by the thread is equal to the action this body exercises on this thread; after all, we have therefore used the principle of the equality of action and reaction, in considering it, not as an experimental truth, but as the very definition of force.

This definition is just as conventional as Kirchhoff's, but far less general.

All forces are not transmitted by threads (besides, to be able to compare them, they would all have to be transmitted by identical threads). Even if it should be conceded that the earth is attached to the sun by some invisible thread, at least it would be admitted that we have no means of measuring its elongation.

Nine times out of ten, consequently, our definition would be at fault; no sort of sense could be attributed to it, and it would be necessary to fall back on Kirchhoff's.

Why then take this *détour*? You admit a certain definition of force which has a meaning only in certain particular cases. In these cases you verify by experiment that it leads to the law of acceleration. On the strength of this experiment, you then take the law of acceleration as a definition of force in all the other cases.

Would it not be simpler to consider the law of acceleration as a definition in all cases, and to regard the experiments in question, not as verifications of this law, but as verifications of the principle of reaction, or as demonstrating that the deformations of an elastic body depend only on the forces to which this body is subjected?

And this is without taking into account that the conditions under which your definition could be accepted are never fulfilled except imperfectly, that a thread is never without mass, that it is never removed from every force except the reaction of the bodies attached to its extremities.

Andrade's ideas are nevertheless very interesting; if they do not satisfy our logical craving, they make us understand better the historic genesis of the fundamental ideas of mechanics. The reflections they suggest show us how the human mind has raised itself from a naïve anthropomorphism to the present conceptions of science.

We see at the start a very particular and in sum rather crude experiment; at the finish, a law perfectly general, perfectly precise, the certainty of which we regard as absolute. This certainty we ourselves have bestowed upon it voluntarily, so to speak, by looking upon it as a convention.

Are the law of acceleration, the rule of the composition of forces then only arbitrary conventions? Conventions, yes; arbitrary, no; they would be if we lost sight of the experiments which led the creators of the science to adopt them, and which, imperfect as they may be, suffice to justify them. It is well that from time to time our attention is carried back to the experimental origin of these conventions.

CHAPTER VII.

RELATIVE MOTION AND ABSOLUTE MOTION.

THE PRINCIPLE OF RELATIVE MOTION.—The attempt has sometimes been made to attach the law of acceleration to a more general principle. The motion of any system must obey the same laws, whether it be referred to fixed axes, or to movable axes carried along in a rectilinear and uniform motion. This is the principle of relative motion, which forces itself upon us for two reasons: first, the commonest experience confirms it, and second, the contrary hypothesis is singularly repugnant to the mind.

Assume it then, and consider a body subjected to a force; the relative motion of this body, in reference to an observer moving with a uniform velocity equal to the initial velocity of the body, must be identical to what its absolute motion would be if it started from rest. We conclude hence that its acceleration can not depend upon its absolute velocity; the attempt has even been made to derive from this a demonstration of the law of acceleration.

There long were traces of this demonstration in the regulations for the degree B. ès Sc. It is evident that this attempt is idle. The obstacle which prevented our demonstrating the law of acceleration is that we had no definition of force; this obstacle subsists in its entirety, since the principle invoked has not furnished us the definition we lacked.

The principle of relative motion is none the less highly interesting and deserves study for its own sake. Let us first try to enunciate it in a precise manner.

We have said above that the accelerations of the different bodies forming part of an isolated system depend only on their relative velocities and positions, and not on their absolute velocities and positions, provided the movable axes to which the relative motion is referred move uniformly in a straight line. Or, if we prefer, their accelerations depend only on the differences of their veloci-

ties and the differences of their coordinates, and not on the absolute values of these velocities and coordinates.

If this principle is true for relative accelerations, or rather for differences of acceleration, in combining it with the law of reaction we shall thence deduce that it is still true of absolute accelerations.

It then remains to be seen how we may demonstrate that the differences of the accelerations depend only on the differences of the velocities and of the coordinates, or, to speak in mathematical language, that these differences of coordinates satisfy differential equations of the second order.

Can this demonstration be deduced from experiments or from *a priori* considerations?

Recalling what we have said above, the reader can answer for himself.

Thus enunciated, in fact, the principle of relative motion singularly resembles what I called above the generalized principle of inertia; it is not altogether the same thing, since it is a question of the differences of coordinates and not of the coordinates themselves. The new principle teaches us therefore something more than the old, but the same discussion is applicable and would lead to the same conclusions; it is unnecessary to return to it.

NEWTON'S ARGUMENT.—Here we encounter a very important and even somewhat disconcerting question. I have said the principle of relative motion was for us not solely a result of experiment, and that *a priori* every contrary hypothesis would be repugnant to the mind.

But then, why is the principle true only if the motion of the movable axes is rectilinear and uniform? It seems that it ought to impose itself upon us with the same force, if this motion is varied, or at any rate if it reduces to a uniform rotation. Now, in these two cases, the principle is not true. I will not dwell long on the case where the motion of the axes is rectilinear without being uniform; the paradox does not bear a moment's examination. If I am on board, and if the train, striking any obstacle, stops suddenly, I shall be thrown against the seat in front of me, although I have not been directly subjected to any force. There is nothing mysterious in that; if I have undergone the action of no

external force, the train itself has experienced an external impact. There can be nothing paradoxical in the relative motion of two bodies being disturbed when the motion of one or the other is modified by an external cause.

I will pause longer on the case of relative motions referred to axes which rotate uniformly. If the heavens were always covered with clouds, if we had no means of observing the stars, we nevertheless might conclude that the earth turns round; we could learn this from its flattening or again by the Foucault pendulum experiment.

And yet, in this case, would it have any meaning, to say the earth turns round? If there is no absolute space, can one turn without turning in reference to something else? and, on the other hand, how could we admit Newton's conclusion and believe in absolute space?

But it does not suffice to ascertain that all possible solutions are equally repugnant to us; we must analyze, in each case, the reasons for our repugnance, so as to make our choice intelligently. The long discussion which follows will therefore be excused.

Let us resume our fiction: thick clouds hide the stars from men, who can not observe them and are ignorant even of their existence; how shall these men know the earth turns round?

Even more than our ancestors no doubt, they will regard the ground which bears them as fixed and immovable; they will await much longer the advent of a Copernicus. But in the end the Copernicus would come—how?

The students of mechanics in this world would not at first be confronted with an absolute contradiction. In the theory of relative motion, besides real forces, two fictitious forces are met which are called ordinary and compound centrifugal force. Our imaginary scientists could therefore explain everything by regarding these two forces as real, and they would not see therein any contradiction of the generalized principle of inertia, for these forces would depend, the one on the relative positions of the various parts of the system, as real attractions do, the other on their relative velocities, as real frictions do.

Many difficulties, however, would soon awaken their attention; if they succeeded in realizing an isolated system, the center

of gravity of this system would not have an almost rectilinear path. They would invoke, to explain this fact, the centrifugal forces which they would regard as real, and which they would attribute no doubt to the mutual actions of the bodies. Only they would not see these forces become null at great distances, that is to say in proportion as the isolation was better realized; far from it; centrifugal force increases indefinitely with the distance.

This difficulty would seem to them already sufficiently great; and yet it would not stop them long; they would soon imagine some very subtle medium, analogous to our ether, in which all bodies would be immersed and which would exert a repellant action upon them.

But this is not all. Space is symmetric, and yet the laws of motion would not show any symmetry; they would have to distinguish between right and left. It would be seen for instance that cyclones turn always in the same sense, whereas by reason of symmetry these winds should turn indifferently in one sense and in the other. If our scientists by their labor had succeeded in rendering their universe perfectly symmetric, this symmetry would not remain, even though there was no apparent reason why it should be disturbed in one sense rather than in the other.

They would get themselves out of the difficulty doubtless, they would invent something which would be no more extraordinary than the glass spheres of Ptolemy, and so it would go on, complications accumulating, until the long-expected Copernicus sweeps them all away at a single stroke, saying: It is much simpler to assume the earth turns round.

And just as our Copernicus said to us: It is more convenient to suppose the earth turns round, since thus the laws of astronomy are expressible in a much simpler language; this one would say: It is more convenient to suppose the earth turns round, since thus the laws of mechanics are expressible in a much simpler language.

This does not preclude maintaining that absolute space, that is to say the mark to which it would be necessary to refer the earth to know whether it really moves, has no objective existence. Hence, this affirmation: 'the earth turns round' has no meaning, since it can be verified by no experiment; since such an experiment, not only could not be either realized or dreamed by the boldest

Jules Verne, but can not be conceived of without contradiction; or rather these two propositions: 'the earth turns round,' and, 'it is more convenient to suppose the earth turns round' have the same meaning; there is nothing more in the one than in the other.

Perhaps one will not be content even with that, and will find it already shocking that among all the hypotheses, or rather all the conventions we can make on this subject, there is one more convenient than the others.

But if it has been admitted without difficulty when it was a question of the laws of astronomy, why should it be shocking in that which concerns mechanics?

We have seen that the coordinates of bodies are determined by differential equations of the second order, and that so are the differences of these coordinates. This is what we have called the generalized principle of inertia and the principle of relative motion. If the distances of these bodies were determined likewise by equations of the second order, it seems that the mind ought to be entirely satisfied. In what measure does the mind get this satisfaction and why is it not content with it?

To account for this, we had better take a simple example. I suppose a system analogous to our solar system, but where one can not perceive fixed stars foreign to this system, so that astronomers can observe only the mutual distances of the planets and the sun, and not the absolute longitudes of the planets. If we deduce directly from Newton's law the differential equations which define the variation of these distances, these equations will not be of the second order. I mean that if, besides Newton's law, one knew the initial values of these distances and of their derivatives with respect to the time, that would not suffice to determine the values of these same distances at a subsequent instant. There would still be lacking one datum, and this datum might be for instance what astronomers call the area-constant.

But here two different points of view may be taken; we may distinguish two sorts of constants. To the eyes of the physicist the world reduces to a series of phenomena, depending, on the one hand, solely upon the initial phenomena; on the other hand, upon the laws which bind the consequents to the antecedents.

If then observation teaches us that a certain quantity is a constant, we shall have the choice between two conceptions.

Either we shall assume that there is a law requiring this quantity not to vary, but that by chance, at the beginning of the ages, it had, rather than another, this value it has been forced to keep ever since. This quantity might then be called an *accidental* constant.

Or else we shall assume, on the contrary, that there is a law of nature which imposes upon this quantity such a value and not such another.

We shall then have what we may call an *essential* constant.

For example, in virtue of Newton's laws, the duration of the revolution of the earth must be constant. But if it is 366 sidereal days and something over, and not 300 or 400, this is in consequence of I know not what initial chance. This is an accidental constant. If, on the contrary, the exponent of the distance which figures in the expression of the attractive force is equal to -2 and not to -3 , this is not by chance, but because Newton's law requires it. This is an essential constant.

I know not whether this way of giving chance its part is legitimate in itself, and whether this distinction is not somewhat artificial; it is certain at least that, so long as nature shall have secrets, this distinction will be in application extremely arbitrary and always precarious.

As to the area-constant, we are accustomed to regard it as accidental. Is it certain our imaginary astronomers would do the same? If they could have compared two different solar systems, they would have the idea that this constant may have several different values; but my very supposition in the beginning was that their system should appear as isolated, and that they should observe no star foreign to it. Under these conditions, they would see only one single constant which would have a single value absolutely invariable; they would be led without any doubt to regard it as an essential constant.

A word in passing to forestall an objection: the inhabitants of this imaginary world could neither observe nor define the area-constant as we do, since the absolute longitudes escape them; that would not preclude their being quickly led to notice a certain

constant which would introduce itself naturally into their equations and which would be nothing but what we call the area-constant.

But then see what would happen. If the area-constant is regarded as essential, as depending upon a law of nature, to calculate the distances of the planets at any instant it will suffice to know the initial values of these distances and those of their first derivatives. From this new point of view, the distances will be determined by differential equations of the second order.

Yet would the mind of these astronomers be completely satisfied? I do not believe so; first, they would soon perceive that in differentiating their equations and thus raising their order, these equations became much simpler. And above all they would be struck by the difficulty which comes from symmetry. It would be necessary to assume different laws, according as the aggregate of the planets presented the figure of a certain polyhedron or of the symmetric polyhedron, and one would escape from this consequence only by regarding the area-constant as accidental.

I have taken a very special example, since I have supposed astronomers who did not at all consider terrestrial mechanics, and whose view was limited to the solar system. Our universe is more extended than theirs, as we have fixed stars, but still it too is limited, and so we might reason on the totality of our universe as the astronomers on their solar system.

Thus we see that finally we should be led to conclude that the equations which define distances are of an order superior to the second. Why should we be shocked at that, why do we find it perfectly natural for the series of phenomena to depend upon the initial values of the first derivatives of these distances, while we hesitate to admit that they may depend on the initial values of the second derivatives? This can only be because of the habits of mind created in us by the constant study of the generalized principle of inertia and its consequences.

The values of the distances at any instant depend upon their initial values, upon those of their first derivatives and also upon something else. What is this *something else*?

If we will not admit that this may be simply one of the second derivatives, we have only the choice of hypotheses. Either it may

be supposed, as is ordinarily done, that this something else is the absolute orientation of the universe in space, or the rapidity with which this orientation varies; and this supposition may be correct; it is certainly the most convenient solution for geometry; it is not the most satisfactory for the philosopher, because this orientation does not exist.

Or it may be supposed that this something else is the position or the velocity of some invisible body; this has been done by certain persons who have even called it the body alpha, although we are doomed never to know anything of this body but its name. This is an artifice entirely analogous to that of which I spoke at the end of the paragraph devoted to my reflections on the principle of inertia.

But, after all, the difficulty is artificial. Provided the future indications of our instruments can depend only on the indications they have given us or would have given us formerly, this is all that is necessary. Now as to this we may rest easy.

CHAPTER VIII.

ENERGY AND THERMODYNAMICS.

ENERGETICS.—The difficulties inherent in the classic mechanics have led certain minds to prefer a new system they call *energetics*.

Energetics took its rise as an outcome of the discovery of the principle of the conservation of energy. Helmholtz gave it its final form.

It begins by defining two quantities which play the fundamental rôle in this theory. They are *kinetic energy*, or *vis viva*, and *potential energy*.

All the changes which bodies in nature can undergo are regulated by two experimental laws:

1° The sum of kinetic energy and potential energy is constant. This is the principle of the conservation of energy.

2° If a system of bodies is at *A* at the time t_0 and at *B* at the time t_1 , it always goes from the first situation to the second in such a way that the *mean* value of the difference between the two sorts of energy, in the interval of time which separates the two epochs t_0 and t_1 , may be as small as possible.

This is Hamilton's principle, which is one of the forms of the principle of least action.

The energetic theory has the following advantages over the classic theory:

1° It is less incomplete; that is to say, Hamilton's principle and that of the conservation of energy teach us more than the fundamental principles of the classic theory, and exclude certain motions not realized in nature and which would be compatible with the classic theory:

2° It saves us the hypothesis of atoms, which it was almost impossible to avoid with the classic theory.

But it raises in its turn new difficulties:

The definitions of the two sorts of energy would raise difficulties almost as great as those of force and mass in the first

system. Yet they may be gotten over more easily, at least in the simplest cases.

Suppose an isolated system formed of a certain number of material points; suppose these points subjected to forces depending only on their relative position and their mutual distances and independent of their velocities. In virtue of the principle of the conservation of energy, a function of forces must exist.

In this simple case the enunciation of the principle of the conservation of energy is of extreme simplicity. A certain quantity, accessible to experiment, must remain constant. This quantity is the sum of two terms; the first depends only on the position of the material points and is independent of their velocities; the second is proportional to the square of these velocities. This resolution can take place only in a single way.

The first of these terms, which I shall call U , will be the potential energy; the second, which I shall call T , will be the kinetic energy.

It is true that if $T + U$ is a constant, so is any function of $T + U$,

$$\phi(T + U).$$

But this function $\phi(T + U)$ will not be the sum of two terms the one independent of the velocities, the other proportional to the square of these velocities. Among the functions which remain constant, there is only one which enjoys this property, that is $T + U$ (or a linear function of $T + U$, which comes to the same thing, since this linear function may always be reduced to $T + U$ by change of unit and of origin). This then is what we shall call energy; the first terms we shall call potential energy and the second kinetic energy. The definition of the two sorts of energy can therefore be carried through without any ambiguity.

It is the same with the definition of the masses. Kinetic energy, or *vis viva*, is expressed very simply by the aid of the masses and the relative velocities of all the material points, with reference to one of them. These relative velocities are accessible to observation, and, when we know the expression of the kinetic energy as function of these relative velocities, the coefficients of this expression will give us the masses.

Thus, in this simple case, the fundamental ideas may be defined without difficulty. But the difficulties reappear in the more complicated cases and, for instance, if the forces, in lieu of depending only on the distances, depend also on the velocities. For example, Weber supposes the mutual action of two electric molecules to depend not only on their distance, but on their velocity and their acceleration. If material points should attract each other according to an analogous law, U would depend on the velocity, and might contain a term proportional to the square of the velocity.

Among the terms proportional to the squares of the velocities, how distinguish those which come from T or from U ? Consequently, how distinguish the two parts of energy?

But still more; how define energy itself? We no longer have any reason to take as definition $T + U$ rather than any other function of $T + U$, when the property which characterized $T + U$ has disappeared, that, namely, of being the sum of two terms of a particular form.

But this is not all; it is necessary to take account, not only of mechanical energy properly so called, but of the other forms of energy, heat, chemical energy, electric energy, etc. The principle of the conservation of energy should be written:

$$T + U + Q = \text{const.}$$

where T would represent the sensible kinetic energy, U the potential energy of position, depending only on the position of the bodies, Q the internal molecular energy, under the thermal, chemic or electric form.

All would go well if these three terms were absolutely distinct, if T were proportional to the square of the velocities, U independent of these velocities and of the state of the bodies, Q independent of the velocities and of the positions of the bodies and dependent only on their internal state.

The expression for the energy could be resolved only in one single way into three terms of this form.

But this is not the case; consider electrified bodies; the electrostatic energy due to their mutual action will evidently depend upon their charge, that is to say, on their state; but it will equally depend upon their position. If these bodies are in motion, they

will act one upon another electro-dynamically and the electro-dynamic energy will depend not only upon their state and their position, but upon their velocities.

We therefore no longer have any means of making the separation of the terms which should make part of T , of U and of Q , and of separating the three parts of energy.

If $(T + U + Q)$ is constant so is any function $\phi(T + U + Q)$.

If $T + U + Q$ were of the particular form I have above considered, no ambiguity would result; among the functions $\phi(T + U + Q)$ which remain constant, there would only be one of this particular form, and that I should convene to call energy.

But as I have said, this is not rigorously the case; among the functions which remain constant, there is none which can be put rigorously under this particular form; hence, how choose among them the one which should be called energy? We no longer have anything to guide us in our choice.

There only remains for us one enunciation of the principle of the conservation of energy: *There is something which remains constant.* Under this form it is in its turn out of the reach of experiment and reduces to a sort of tautology. It is clear that if the world is governed by laws, there will be quantities which will remain constant. Like Newton's laws, and, for an analogous reason, the principle of the conservation of energy, founded on experiment, could no longer be invalidated by it.

This discussion shows that in passing from the classic to the energetic system, progress has been made; but at the same time it shows this progress is insufficient.

Another objection seems to me still more grave: the principle of least action is applicable to reversible phenomena; but it is not at all satisfactory in so far as irreversible phenomena are concerned; the attempt by Helmholtz to extend it to this kind of phenomena did not succeed and could not succeed; in this regard everything remains to be done. The very statement of the principle of least action has something about it repugnant to the mind. To go from one point to another, a material molecule, acted upon by no force, but required to move on a surface, will take the geodesic line, that is to say the shortest path.

This molecule seems to know the point whither it is to go,

to foresee the time it would take to reach it by such and such a route, and then to choose the most suitable path. The statement presents the molecule to us, so to speak, as a living and free being. Clearly it would be better to replace it by an enunciation less objectionable, and where, as the philosophers would say, final causes would not seem to be substituted for efficient causes.

THERMODYNAMICS.*—The rôle of the two fundamental principles of thermodynamics in all branches of natural philosophy becomes daily more important. Abandoning the ambitious theories of forty years ago, which were encumbered by molecular hypotheses, we are trying to-day to erect upon thermodynamics alone the entire edifice of mathematical physics. Will the two principles of Mayer and of Clausius assure to it foundations solid enough for it to last some time? No one doubts it; but whence comes this confidence?

An eminent physicist said to me one day *à propos* of the law of errors: "All the world believes it firmly, because the mathematicians imagine that it is a fact of observation, and the observers that it is a theorem of mathematics." It was long so for the principle of the conservation of energy. It is no longer so to-day; no one is ignorant that this is an experimental fact.

But then what gives us the right to attribute to the principle itself more generality and more precision than to the experiments which have served to demonstrate it? This is to ask whether it is legitimate, as is done every day, to generalize empirical data, and I shall not have the presumption to discuss this question, after so many philosophers have vainly striven to solve it. One thing is certain; if this power were denied us, science could not exist or, at least, reduced to a sort of inventory, to the ascertaining of isolated facts, it would have no value for us, since it could give no satisfaction to our craving for order and harmony and since it would be at the same time incapable of foreseeing. As the circumstances which have preceded any fact will probably never be simultaneously reproduced, a first generalization is already necessary to foresee whether this fact will be reproduced again after the least of these circumstances shall be changed.

* The following lines are a partial reproduction of the preface of my book *Thermodynamique*.

But every proposition may be generalized in an infinity of ways. Among all the generalizations possible, we must choose, and we can only choose the simplest. We are therefore led to act as if a simple law were, other things being equal, more probable than a complicated law.

Half a century ago this was frankly confessed, and it was proclaimed that nature loves simplicity; she has since too often given us the lie. To-day we no longer confess this tendency, and we retain only so much of it as is indispensable if science is not to become impossible.

In formulating a general, simple and precise law on the basis of experiments relatively few and presenting certain divergences, we have therefore only obeyed a necessity from which the human mind can not free itself.

But there is something more, and this is why I dwell upon the point.

No one doubts that Mayer's principle is destined to survive all the particular laws from which it was obtained, just as Newton's law has survived Kepler's laws, from which it sprang, and which are only approximative if account be taken of perturbations.

Why does this principle occupy thus a sort of privileged place among all the physical laws? There are many little reasons for it.

First of all it is believed that we could not reject it or even doubt its absolute rigor without admitting the possibility of perpetual motion; of course we are on our guard at such a prospect, and we think ourselves less rash in affirming Mayer's principle than in denying it.

That is perhaps not wholly accurate; the impossibility of perpetual motion implies the conservation of energy only for reversible phenomena.

The imposing simplicity of Mayer's principle likewise contributes to strengthen our faith. In a law deduced immediately from experiment, like Mariotte's, this simplicity would rather seem to us a reason for distrust; but here this is no longer the case; we see elements, at first sight disparate, arrange themselves in an unexpected order and form a harmonious whole; and we refuse to believe that an unforeseen harmony may be a simple effect of chance. It seems that our conquest is the dearer to us the more

effort it has cost us, or that we are the surer of having wrested her true secret from nature the more jealously she has hidden it from us.

But those are only little reasons; to establish Mayer's law as an absolute principle, a more profound discussion is necessary. But if this be attempted, it is seen that this absolute principle is not even easy to state.

In each particular case it is clearly seen what energy is and at least a provisional definition of it can be given; but it is impossible to find a general definition for it.

If we try to enunciate the principle in all its generality and apply it to the universe, we see it vanish, so to speak, and nothing is left but this: *There is something which remains constant.*

But has even this any meaning? In the determinist hypothesis, the state of the universe is determined by an extremely great number n of parameters which I shall call $x_1, x_2, \dots x_n$. As soon as the values of these n parameters at any instant are known, their derivatives with respect to the time are likewise known and consequently the values of these same parameters at a preceding or subsequent instant can be calculated. In other words, these n parameters satisfy n differential equations of the first order.

These equations admit of $n - 1$ integrals and consequently there are $n - 1$ functions of $x_1, x_2, \dots x_n$, which remain constant. *If then we say there is something which remains constant*, we only utter a tautology. We should even be puzzled to say which among all our integrals should retain the name of energy.

Besides, Mayer's principle is not understood in this sense when it is applied to a limited system. It is then assumed that p of our parameters vary independently, so that we only have $n - p$ relations, generally linear, between our n parameters and their derivatives.

To simplify the enunciation, suppose that the sum of the work of the external forces is null, as well as that of the quantities of heat given off to the outside. Then the signification of our principle will be:

There is a combination of these $n - p$ relations whose first member is an exact differential; and then this differential vanish-

ing in virtue of our $n - p$ relations, its integral is a constant and this integral is called energy.

But how can it be possible that there are several parameters whose variations are independent? That can only happen under the influence of external forces (although we have supposed, for simplicity, that the algebraic sum of the effects of these forces is null). In fact, if the system were completely isolated from all external action, the values of our n parameters at a given instant would suffice to determine the state of the system at any subsequent instant, provided always we retain the determinist hypothesis; we come back therefore to the same difficulty as above.

If the future state of the system is not entirely determined by its present state, this is because it depends besides upon the state of bodies external to the system. But then is it probable that there exist between the parameters x , which define the state of the system, equations independent of this state of the external bodies? and if in certain cases we believe we can find such, is this not solely in consequence of our ignorance and because the influence of these bodies is too slight for our experimenting to detect it?

If the system is not regarded as completely isolated, it is probable that the rigorously exact expression of its internal energy will depend on the state of the external bodies. Again, I have above supposed the sum of the external work was null, and if we try to free ourselves from this rather artificial restriction, the enunciation becomes still more difficult.

To formulate Mayer's principle in an absolute sense, it is therefore necessary to extend it to the whole universe, and then we find ourselves face to face with the very difficulty we sought to avoid.

In conclusion, using ordinary language, the law of the conservation of energy can have only one signification, which is that there is a property common to all the possibilities; but on the determinist hypothesis there is only a single possibility, and then the law has no longer any meaning.

On the indeterminist hypothesis, on the contrary, it would have a meaning, even if it were taken in an absolute sense; it would appear as a limitation imposed upon freedom.

But this word reminds me that I am digressing and am on

the point of leaving the domain of mathematics and physics. I check myself therefore and will stress of all this discussion only one impression, that Mayer's law is a form flexible enough for us to put into it almost whatever we wish. By that I do not mean it corresponds to no objective reality, nor that it reduces itself to a mere tautology, since, in each particular case, and provided one does not try to push to the absolute, it has a perfectly clear meaning.

This flexibility is a reason for believing in its permanence, and as, on the other hand, it will disappear only to lose itself in a higher harmony, we may work with confidence, supporting ourselves upon it, certain beforehand that our labor will not be lost.

Almost everything I have just said applies to the principle of Clausius. What distinguishes it is, that it is expressed by an inequality. Perhaps it will be said it is the same with all physical laws, since their precision is always limited by errors of observation. But they at least claim to be first approximations, and it is hoped to replace them little by little by laws more and more precise. If, on the other hand, the principle of Clausius reduces to an inequality, this is not caused by the imperfection of our means of observation, but by the very nature of the question.

GENERAL CONCLUSIONS ON PART THIRD.

The principles of mechanics, then, present themselves to us under two different aspects. On the one hand, they are truths founded on experiment and approximately verified so far as concerns almost isolated systems. On the other hand, they are postulates applicable to the totality of the universe and regarded as rigorously true.

If these postulates possess a generality and a certainty which are lacking to the experimental verities whence they are drawn, this is because they reduce in the last analysis to a mere convention which we have the right to make, because we are certain beforehand that no experiment can ever contradict it.

This convention, however, is not absolutely arbitrary; it does not spring from our caprice; we adopt it because certain experiments have shown us that it would be convenient.

Thus is explained how experiment could make the principles of mechanics, and yet why it can not overturn them.

Compare with geometry: The fundamental propositions of geometry, as for instance Euclid's postulate, are nothing more than conventions, and it is just as unreasonable to inquire whether they are true or false as to ask whether the metric system is true or false.

Only, these conventions are convenient, and it is certain experiments which have taught us that.

At first blush, the analogy is complete; the rôle of experiment seems the same. One will therefore be tempted to say: Either mechanics must be regarded as an experimental science, and then the same must hold for geometry; or else, on the contrary, geometry is a deductive science, and then one may say as much of mechanics.

Such a conclusion would be illegitimate. The experiments which have led us to adopt as more convenient the fundamental conventions of geometry bear on objects which have nothing in common with those geometry studies; they bear on the properties of solid bodies, on the rectilinear propagation of light. They are experiments of mechanics, experiments of optics; they can not in any way be regarded as experiments of geometry. And even the principle reason why our geometry seems convenient to us is that the different parts of our body, our eye, our limbs, have the properties of solid bodies. On this account, our fundamental experiments are preeminently physiological experiments, which bear, not on space which is the object the geometer must study, but on his body, that is to say, on the instrument he must use for this study.

On the contrary, the fundamental conventions of mechanics, and the experiments which prove to us that they are convenient, bear on exactly the same objects or on analogous objects. The conventional and general principles are the natural and direct generalization of the experimental and particular principles.

Let it not be said that thus I trace artificial frontiers between the sciences; that if I separate by a barrier geometry properly so called from the study of solid bodies, I could just as well erect one between experimental mechanics and the conventional me-

chanics of the general principles. In fact, who does not see that in separating these two sciences I mutilate them both, and that what will remain of conventional mechanics when it shall be isolated will be only a very small thing and can in no way be compared to that superb body of doctrine called geometry?

One sees now why the teaching of mechanics should remain experimental.

Only thus can it make us comprehend the genesis of the science, and that is indispensable for the complete understanding of the science itself.

Besides, if we study mechanics, it is to apply it; and we can apply it only if it remains objective. Now, as we have seen, what the principles gain in generality and certainty, they lose in objectivity. It is therefore above all with the objective side of the principles that we must be familiarized early, and that can be done only by going from the particular to the general, instead of the inverse.

The principles are conventions and disguised definitions. Yet they are drawn from experimental laws; these laws have been so to speak exalted into principles to which our mind attributes an absolute value.

Some philosophers have generalized too far; they believed the principles were the whole science and consequently that the whole science was conventional.

This paradoxical doctrine, called nominalism, will not bear examination.

How can a law become a principle? It expressed a relation between two real terms A and B . But it was not rigorously true, it was only approximate. We introduce arbitrarily an intermediary term C more or less fictitious, and C is *by definition* that which has with A *exactly* the relation expressed by the law.

Then our law is separated into an absolute and rigorous principle which expresses the relation of A to C and an experimental law approximate and subject to revision which expresses the relation of C to B . It is clear that, however far this partition is pushed, some laws will always be left remaining.

We go to enter now the domain of laws properly so called.

PART IV.

NATURE.

CHAPTER IX.

HYPOTHESES IN PHYSICS.

THE RÔLE OF EXPERIMENT AND GENERALIZATION.—Experiment is the sole source of truth. It alone can teach us anything new; it alone can give us certainty. These are two points that can not be questioned.

But then, if experiment is everything, what place will remain for mathematical physics? What has experimental physics to do with such an aid, one which seems useless and perhaps even dangerous?

And yet mathematical physics exists, and has done unquestionable service. We have here a fact that must be explained.

The explanation is that merely to observe is not enough. We must use our observations, and to do that we must generalize. This is what men always have done; only as the memory of past errors has made them more and more careful, they have observed more and more, and generalized less and less.

Every age has ridiculed the one before it, and accused it of having generalized too quickly and too naïvely. Descartes pitied the Ionians; Descartes, in his turn, makes us smile. No doubt our children will some day laugh at us.

But can we not then pass over immediately to the goal? Is not this the means of escaping the ridicule that we foresee? Can we not be content with just the bare experiment?

No, that is impossible; it would be to mistake utterly the true nature of science. The scientist must set in order. Science is built up with facts, as a house is with stones. But a collection of facts is no more a science than a heap of stones is a house.

And above all the scientist must foresee. Carlyle has some-

where said something like this: "Nothing but facts are of importance. John Lackland passed by here. Here is something that is admirable. Here is a reality for which I would give all the theories in the world." Carlyle was a fellow countryman of Bacon; but Bacon would not have said that. That is the language of the historian. The physicist would say rather: "John Lackland passed by here; that makes no difference to me, for he never will pass this way again."

We all know that there are good experiments and poor ones. The latter will accumulate in vain; though one may have made a hundred or a thousand, a single piece of work by a true master, by a Pasteur, for example, will suffice to tumble them into oblivion. Bacon would have well understood this; it is he who invented the phrase *Experimentum crucis*. But Carlyle would not have understood it. A fact is a fact. A pupil has read a certain number on his thermometer; he has taken no precaution; no matter, he has read it, and if it is only the fact that counts, here is a reality of the same rank as the peregrinations of King John Lackland. Why is the fact that this pupil has made this reading of no interest, while the fact that a skilled physicist had made another reading might be on the contrary very important? It is because from the first reading we could not infer anything. What then is a good experiment? It is that which informs us of something besides an isolated fact; it is that which enables us to foresee, that is, that which enables us to generalize.

For without generalization foreknowledge is impossible. The circumstances under which one has worked will never reproduce themselves all at once. The observed action then will never recur; the only thing that can be affirmed is that under analogous circumstances an analogous action will be produced. In order to foresee, then, it is necessary to invoke at least analogy, that is to say, already then to generalize.

No matter how timid one may be, still it is necessary to interpolate. Experiment gives us only a certain number of isolated points. We must unite these by a continuous line. This is a veritable generalization. But we do more; the curve that we shall trace will pass between the observed points and near these points; it will not pass through these points themselves. Thus one does

not restrict himself to generalizing the experiments, but corrects them; and the physicist who should try to abstain from these corrections and really be content with the bare experiment, would be forced to enunciate some very strange laws.

The bare facts, then, would not be enough for us; and that is why we must have science ordered, or rather organized.

It is often said experiments must be made without a preconceived idea. That is impossible. Not only would it make all experiment barren, but that would be attempted which could not be done. Every one carries in his mind his own conception of the world, of which he can not so easily rid himself. We must, for instance, use language; and our language is made up only of preconceived ideas and can not be otherwise. Only these are unconscious preconceived ideas, a thousand times more dangerous than the others.

Shall we say that if we introduce others, of which we are fully conscious, we shall only aggravate the evil? I think not. I believe rather that they will serve as counterbalances to each other—I was going to say as antidotes; they will in general accord ill with one another; they will come into conflict with one another, and thereby force us to regard things under different aspects. This is enough to emancipate us. He is no longer a slave who can choose his master.

Thus, thanks to generalization, each fact observed enables us to foresee a great many others; only we must not forget that the first alone is certain, that all others are merely probable. No matter how solidly founded a prediction may appear to us, we are never *absolutely* sure that experiment will not contradict it, if we undertake to verify it. The probability, however, is often so great that practically we may be content with it. It is far better to foresee even without certainty than not to foresee at all.

One must, then, never disdain to make a verification when opportunity offers. But all experiment is long and difficult; the workers are few; and the number of facts that we need to foresee is immense. Compared with this mass the number of direct verifications that we can make will never be anything but a negligible quantity.

Of this few that we can directly attain, we must make the best

use; it is very necessary to get from every experiment the greatest possible number of predictions, and with the highest possible degree of probability. The problem is, so to speak, to increase the yield of the scientific machine.

Let us compare science to a library that ought to grow continually. The librarian has at his disposal for his purchases only insufficient funds. He ought to make an effort not to waste them.

It is experimental physics that is entrusted with the purchases. It alone, then, can enrich the library.

As for mathematical physics, its task will be to make out the catalogue. If the catalogue is well made, the library will not be any richer, but the reader will be helped to use its riches.

And even by showing the librarian the gaps in his collections, it will enable him to make a judicious use of his funds; which is all the more important because these funds are entirely inadequate.

Such, then, is the rôle of mathematical physics. It must direct generalization in such a manner as to increase what I just now called the yield of science. By what means it can arrive at this, and how it can do it without danger, is what remains for us to investigate.

THE UNITY OF NATURE.—Let us notice first of all, that every generalization implies in some measure the belief in the unity and simplicity of nature. As to the unity there can be no difficulty. If the different parts of the universe were not like the members of one body, they would not act on one another, they would know nothing of each other; and we in particular would know only one of these parts. We do not ask, then, if nature is one, but how it is one.

As for the second point, that is not such an easy matter. It is not certain that nature is simple. Can we without danger act as if it were?

There was a time when the simplicity of Mariotte's law was an argument invoked in favor of its accuracy; when Fresnel himself, after having said in a conversation with Laplace that nature was not concerned about analytical difficulties, felt himself obliged to make explanations, in order not to strike too hard at prevailing opinion.

To-day ideas have greatly changed; and yet, those who do

not believe that natural laws have to be simple, are still often obliged to act as if they did. They could not entirely avoid this necessity without making impossible all generalization, and consequently all science.

It is clear that any fact can be generalized in an infinity of ways, and it is a question of choice. The choice can be guided only by considerations of simplicity. Let us take the most commonplace case, that of interpolation. We pass a continuous line, as regular as possible, between the points given by observation. Why do we avoid points making angles, and too abrupt turns? Why do we not make our curve describe the most capricious zig-zags? It is because we know beforehand, or believe we know, that the law to be expressed can not be so complicated as all that.

We may calculate the mass of Jupiter from either the movements of its satellites, or the perturbations of the major planets, or those of the minor planets. If we take the averages of the determinations obtained by these three methods, we find three numbers very close together, but different. We might interpret this result by supposing that the coefficient of gravitation is not the same in the three cases. The observations would certainly be much better represented. Why do we reject this interpretation? Not because it is absurd, but because it is needlessly complicated. We shall only accept it when we are forced to, and that is not yet.

To sum up, ordinarily every law is held to be simple till the contrary is proved.

This custom is imposed upon physicists by the causes that I have just explained. But how shall we justify it in the presence of discoveries that show us every day new details that are richer and more complex? How shall we even reconcile it with the belief in the unity of nature? For if everything depends on everything, relationships where so many diverse factors enter can no longer be simple.

If we study the history of science, we see happen two inverse phenomena, so to speak. Sometimes simplicity hides under complex appearances; sometimes it is the simplicity which is apparent, and which disguises extremely complicated realities.

What is more complicated than the confused movements of the planets? What simpler than Newton's law? Here nature,

making sport, as Fresnel said, of analytical difficulties, employs only simple means, and by combining them produces I know not what inextricable tangle. Here it is the hidden simplicity which must be discovered.

Examples of the opposite abound. In the kinetic theory of gases, one deals with molecules moving with great velocities, whose paths, altered by incessant collisions, have the most capricious forms and traverse space in every direction. The observable result is Mariotte's simple law. Every individual fact was complicated. The law of great numbers has reestablished simplicity in the average. Here the simplicity is merely apparent, and only the coarseness of our senses prevents our perceiving the complexity.

Many phenomena obey a law of proportionality. But why? Because in these phenomena there is something very small. The simple law observed, then, is only a result of the general analytical rule that the infinitely small increment of a function is proportional to the increment of the variable. As in reality our increments are not infinitely small, but very small, the law of proportionality is only approximate, and the simplicity is only apparent. What I have just said applies to the rule of the superposition of small motions, the use of which is so fruitful, and which is the basis of optics.

And Newton's law itself? Its simplicity, so long undetected, is perhaps only apparent. Who knows whether it is not due to some complicated mechanism, to the impact of some subtle matter animated by irregular movements, and whether it has not become simple only through the action of averages and of great numbers? In any case, it is difficult not to suppose that the true law contains complementary terms, which would become sensible at small distances. If in astronomy they are negligible as modifying Newton's law, and if the law thus regains its simplicity, it would be only because of the immensity of celestial distances.

No doubt, if our means of investigation should become more and more penetrating, we should discover the simple under the complex, then the complex under the simple, then again the simple under the complex, and so on, without our being able to foresee what will be the last term.

We must stop somewhere, and that science may be possible, we

must stop when we have found simplicity. This is the only ground on which we can rear the edifice of our generalizations. But this simplicity being only apparent, will the ground be firm enough? This is what must be investigated.

For that purpose, let us see what part is played in our generalizations by the belief in simplicity. We have verified a simple law in a good many particular cases; we refuse to admit that this agreement, so often repeated, is simply the result of chance, and conclude that the law must be true in the general case.

Kepler notices that a planet's positions, as observed by Tycho, are all on one ellipse. Never for a moment does he have the thought that by a strange play of chance, Tycho never observed the heavens except at a moment when the real orbit of the planet happened to cut this ellipse.

What does it matter then whether the simplicity be real, or whether it covers a complex reality? Whether it is due to the influence of great numbers, which levels down individual differences, or to the greatness or smallness of certain quantities, which allows us to neglect certain terms, in no case is it due to chance. This simplicity, real or apparent, always has a cause. We can always follow, then, the same course of reasoning, and if a simple law has been observed in several particular cases, we can legitimately suppose that it will still be true in analogous cases. To refuse to do this would be to attribute to chance an inadmissible rôle.

There is, however, a difference. If the simplicity were real and essential, it would resist the increasing precision of our means of measure. If then we believe nature to be essentially simple, we must, from a simplicity that is approximate, infer a simplicity that is rigorous. This is what was done formerly; and this is what we no longer have a right to do.

The simplicity of Kepler's laws, for example, is only apparent. That does not prevent their being applicable, very nearly, to all systems analogous to the solar system; but it does prevent their being rigorously exact.

THE RÔLE OF HYPOTHESIS.—All generalization is a hypothesis. Hypothesis, then, has a necessary rôle that no one has ever contested. Only, it ought always, as soon as possible and as often

as possible, to be subjected to verification. And, of course, if it does not stand this test, it ought to be abandoned without reserve. This is what we generally do, but sometimes with rather an ill humor.

Well, even this ill humor is not justified. The physicist who has just renounced one of his hypotheses ought, on the contrary, to be full of joy; for he has found an unexpected opportunity for discovery. His hypothesis, I imagine, had not been adopted without consideration; it took account of all the known factors that it seemed could enter into the phenomenon. If the test does not support it, it is because there is something unexpected and extraordinary; and because there is going to be something found that is unknown and new.

Has the discarded hypothesis, then, been barren? Far from that, it may be said it has rendered more service than a true hypothesis. Not only has it been the occasion of the decisive experiment, but, without having made the hypothesis, the experiment would have been made by chance, so that nothing would have been derived from it. One would have seen nothing extraordinary; only one fact the more would have been catalogued without deducing from it the least consequence.

Now on what condition is the use of hypothesis without danger?

The firm determination to submit to experiment is not enough; there are still dangerous hypotheses; first, and above all, those which are tacit and unconscious. Since we make them without knowing it, we are powerless to abandon them. Here again, then, is a service that mathematical physics can render us. By the precision that is characteristic of it, it compels us to formulate all the hypotheses that we should make without it, but unconsciously.

Let us notice besides that it is important not to multiply hypotheses beyond measure, and to make them only one after the other. If we construct a theory based on a number of hypotheses, and if experiment condemns it, which of our premises is it necessary to change? It will be impossible to know. And inversely, if the experiment succeeds, shall we believe that we have demonstrated all the hypotheses at once? Shall we believe that with one single equation we have determined several unknowns?

We must equally take care to distinguish between the different kinds of hypotheses. There are first those which are perfectly natural and from which one can scarcely escape. It is difficult not to suppose that the influence of bodies very remote is quite negligible, that small movements follow a linear law, that the effect is a continuous function of its cause. I will say as much of the conditions imposed by symmetry. All these hypotheses form, as it were, the common basis of all the theories of mathematical physics. They are the last that ought to be abandoned.

There is a second class of hypotheses, that I shall term neutral. In most questions the analyst assumes at the beginning of his calculations either that matter is continuous or, on the contrary, that it is formed of atoms. He might have made the opposite assumption without changing his results. He would only have had more trouble to obtain them; that is all. If, then, experiment confirms his conclusions, will he think that he has demonstrated, for instance, the real existence of atoms?

In optical theories two vectors are introduced, of which one is regarded as a velocity, the other as a vortex. Here again is a neutral hypothesis, since the same conclusions would have been reached by taking precisely the opposite. The success of the experiment, then, can not prove that the first vector is indeed a velocity; it can only prove one thing, that it is a vector. This is the only hypothesis that has really been introduced in the premises. In order to give it that concrete appearance which the weakness of our minds requires, it has been necessary to consider it either as a velocity or as a vortex, in the same way that it has been necessary to represent it by a letter, either x or y . The result, however, whatever it may be, will not prove that it was right or wrong to regard it as a velocity any more than it will prove that it was right or wrong to call it x and not y .

These neutral hypotheses are never dangerous, if only their character is not misunderstood. They may be useful, either as devices for computation, or to aid our understanding by concrete images, to fix our ideas as the saying is. There is, then, no occasion to exclude them.

The hypotheses of the third class are the real generalizations. They are the ones that experiment must confirm or invalidate.

Whether verified or condemned, they will always be fruitful. But for the reasons that I have set forth, they will only be fruitful if they are not too numerous.

ORIGIN OF MATHEMATICAL PHYSICS.—Let us penetrate further, and study more closely the conditions that have permitted the development of mathematical physics. We observe at once that the efforts of scientists have always aimed to resolve the complex phenomenon directly given by experiment into a very large number of elementary phenomena.

This is done in three different ways: first, in time. Instead of embracing in its entirety the progressive development of a phenomenon, the aim is simply to connect each instant with the instant immediately preceding it. It is admitted that the actual state of the world depends only on the immediate past, without being directly influenced, so to speak, by the memory of a distant past. Thanks to this postulate, instead of studying directly the whole succession of phenomena, it is possible to confine ourselves to writing its 'differential equation.' For Kepler's laws we substitute Newton's law.

Next we try to analyze the phenomenon in space. What experiment gives us is a confused mass of facts presented on a stage of considerable extent. We must try to discover the elementary phenomenon, which will be, on the contrary, localized in a very small region of space.

Some examples will perhaps make my thought better understood. If we wished to study in all its complexity the distribution of temperature in a cooling solid, we could never succeed. Everything becomes simple if we reflect that one point of the solid can not give up its heat directly to a distant point; it will give up its heat only to the points in the immediate neighborhood, and it is by degrees that the flow of heat can reach other parts of the solid. The elementary phenomenon is the exchange of heat between two contiguous points. It is strictly localized, and is relatively simple, if we admit, as is natural, that it is not influenced by the temperature of molecules whose distance is sensible.

I bend a rod. It is going to take a very complicated form, the direct study of which would be impossible. But I shall be able, however, to attack it, if I observe that its flexure is a result

only of the deformation of the very small elements of the rod, and that the deformation of each of these elements depends only on the forces that are directly applied to it, and not at all on those which may act on the other elements.

In all these examples, which I might easily multiply, we admit that there is no action at a distance, or at least at a great distance. This is a hypothesis. It is not always true, as the law of gravitation shows us. It must, then, be submitted to verification. If it is confirmed, even approximately, it is precious, for it will enable us to make mathematical physics, at least by successive approximations.

If it does not stand the test, we must look for something else analogous; for there are still other means of arriving at the elementary phenomenon. If several bodies act simultaneously, it may happen that their actions are independent and are simply added to one another, either as vectors or as scalars. The elementary phenomenon is then the action of an isolated body. Or again, we have to deal with small movements, or more generally, with small variations, which obey the well-known law of superposition. The observed movement will then be decomposed into simple movements, for example, sound into its harmonics, white light into its monochromatic components.

When we have discovered in what direction it is advisable to look for the elementary phenomenon, by what means can we reach it?

First of all, it will often happen that in order to detect it, or rather to detect the part of it useful to us, it will not be necessary to penetrate the mechanism; the law of great numbers will suffice.

Let us take again the instance of the propagation of heat. Every molecule emits rays toward every neighboring molecule. According to what law, we do not need to know. If we should make any supposition in regard to this, it would be a neutral hypothesis and consequently useless and incapable of verification. And, in fact, by the action of averages and thanks to the symmetry of the medium, all the differences are leveled down, and whatever hypothesis may be made, the result is always the same.

The same circumstance is presented in the theory of electricity

and in that of capillarity. The neighboring molecules attract and repel one another. We do not need to know according to what law; it is enough for us that this attraction is sensible only at small distances, that the molecules are very numerous, that the medium is symmetrical, and we shall only have to let the law of great numbers act.

Here again the simplicity of the elementary phenomenon was hidden under the complexity of the resultant observable phenomenon; but, in its turn, this simplicity was only apparent, and concealed a very complex mechanism.

The best means of arriving at the elementary phenomenon would evidently be experiment. We ought by experimental contrivance to dissociate the complex sheaf that nature offers to our researches, and to study with care the elements as much isolated as possible. For example, natural white light would be decomposed into monochromatic lights by the aid of the prism, and into polarized lights by the aid of the polarizer.

Unfortunately that is neither always possible nor always sufficient, and sometimes the mind must outstrip experiment. I shall cite only one example, which has always struck me forcibly.

If I decompose white light, I shall be able to isolate a small part of the spectrum, but however small it may be, it will retain a certain breadth. Likewise the natural lights called *monochromatic*, give us a very narrow line, but not, however, infinitely narrow. It might be supposed that by studying experimentally the properties of these natural lights, by working with finer and finer lines of the spectrum, and by passing at last to the limit, so to speak, we should succeed in learning the properties of a light strictly monochromatic.

That would not be accurate. Suppose that two rays emanate from the same source, that we polarize them first in two perpendicular planes, then bring them back to the same plane of polarization, and try to make them interfere. If the light were *strictly* monochromatic, they would interfere. With our lights, which are nearly monochromatic, there will be no interference, and that no matter how narrow the line. In order to be otherwise it would have to be several million times as narrow as the finest known lines.

Here, then, the passage to the limit would have deceived us. The mind must outstrip the experiment, and if it has done so with success, it is because it has allowed itself to be guided by the instinct of simplicity.

The knowledge of the elementary fact enables us to put the problem in an equation. Nothing remains but to deduce from this by combination the complex fact that can be observed and verified. This is what is called *integration*, and is the business of the mathematician.

It may be asked why, in physical sciences, generalization so readily takes the mathematical form. The reason is now easy to see. It is not only because we have numerical laws to express; it is because the observable phenomenon is due to the superposition of a great number of elementary phenomena *all alike*. Thus quite naturally are introduced differential equations.

It is not enough that each elementary phenomenon obeys simple laws; all those to be combined must obey the same law. Then only can the intervention of mathematics be of use; mathematics teaches us in fact to combine like with like. Its aim is to learn the result of a combination without needing to go over the combination piece by piece. If we have to repeat several times the same operation, it enables us to avoid this repetition by telling us in advance the result of it by a sort of induction. I have explained this above, in the chapter on mathematical reasoning.

But, for this, all the operations must be alike. In the opposite case, it would evidently be necessary to resign ourselves to doing them in reality one after another, and mathematics would become useless.

It is then thanks to the approximate homogeneity of the matter studied by physicists, that mathematical physics could be born.

In the natural sciences, we no longer find these conditions: homogeneity, relative independence of remote parts, simplicity of the elementary fact; and this is why naturalists are obliged to resort to other methods of generalization.

CHAPTER X.

THE THEORIES OF MODERN PHYSICS.

MEANING OF PHYSICAL THEORIES.—The laity are struck to see how ephemeral scientific theories are. After some years of prosperity, they see them successively abandoned; they see ruins accumulate upon ruins; they foresee that the theories fashionable to-day will shortly succumb in their turn and hence they conclude that these are absolutely idle. This is what they call the *bankruptcy of science*.

Their scepticism is superficial; they give no account to themselves of the aim and the rôle of scientific theories; otherwise they would comprehend that the ruins may still be good for something.

No theory seemed more solid than that of Fresnel which attributed light to motions of the ether. Yet now Maxwell's is preferred. Does this mean the work of Fresnel was in vain? No, because the aim of Fresnel was not to find out whether there is really an ether, whether it is or is not formed of atoms, whether these atoms really move in this or that sense; his object was to foresee optical phenomena.

Now, Fresnel's theory always permits of this, to-day as well as before Maxwell. The differential equations are always true; they can always be integrated by the same procedures and the results of this integration always retain their value.

And let no one say that thus we reduce physical theories to the rôle of mere practical recipes; these equations express relations, and if the equations remain true it is because these relations preserve their reality. They teach us, now as then, that there is such and such a relation between some thing and some other thing; only this something formerly we called *motion*; we now call it *electric current*. But these appellations were only images substituted for the real objects which nature will eternally hide from us. The true relations between these real objects are the only reality we can attain to, and the only condition is that

the same relations exist between these objects as between the images by which we are forced to replace them. If these relations are known to us, what matter if we deem it convenient to replace one image by another.

That some periodic phenomenon (an electric oscillation, for instance) is really due to the vibration of some atom which, acting like a pendulum, really moves in this or that sense, is neither certain nor interesting. But that between electric oscillation, the motion of the pendulum and all periodic phenomena there exists a close relationship which corresponds to a profound reality; that this relationship, this similitude, or rather this parallelism extends into details; that it is a consequence of more general principles, that of energy and that of least action; this is what we can affirm; this is the truth which will always remain the same under all the costumes in which we may deem it useful to deck it out.

Numerous theories of dispersion have been proposed; the first was imperfect and contained only a small part of truth. Afterward came that of Helmholtz; then it was modified in various ways, and its author himself imagined another founded on the principles of Maxwell. But, what is remarkable, all the scientists who came after Helmholtz reached the same equations, starting from points of departure in appearance very widely separated. I will venture to say these theories are all true at the same time, not only because they make us foresee the same phenomena, but because they put in evidence a true relation, that of absorption and anomalous dispersion. What is true in the premises of these theories is what is common to all the authors; this is the affirmation of this or that relation between certain things which some call by one name, others by another.

The kinetic theory of gases has given rise to many objections, which we could hardly answer if we pretended to see in it the absolute truth. But all these objections will not preclude its having been useful, and particularly so in revealing to us a relation true and but for it profoundly hidden, that of the gaseous pressure and the osmotic pressure. In this sense, then, it may be said to be true.

When a physicist finds a contradiction between two theories equally dear to him, he sometimes says: "We will not bother about

that, but hold firmly the two ends of the chain though the intermediate links are hidden from us." This argument of an embarrassed theologian would be ridiculous if it were necessary to attribute to physical theories the sense the laity give them. In case of contradiction, one of them at least must then be regarded as false. It is no longer the same if in them be sought only what should be sought. May-be they both express true relations and the contradiction is only in the images wherewith we have clothed the reality.

To those who find we restrict too much the domain accessible to the scientist, I answer: These questions which we interdict to you and which you regret, are not only insoluble, they are illusory and devoid of meaning.

Some philosopher pretends that all physics may be explained by the mutual impacts of atoms. If he merely means there are between physical phenomena the same relations as between the mutual impacts of a great number of balls, well and good, that is verifiable, that is perhaps true. But he means something more; and we think we understand it because we think we know what impact is in itself; why? Simply because we have often seen games of billiards. Shall we think God, contemplating his work, feels the same sensations as we in watching a billiard match? If we do not wish to give this bizarre sense to his assertion, if neither do we wish the restricted sense I have just explained, which is good sense, then it has none.

Hypotheses of this sort have therefore only a metaphorical sense. The scientist should no more interdict them than the poet does metaphors; but he ought to know what they are worth. They may be useful to give a certain satisfaction to the mind, and they will not be injurious provided they are only indifferent hypotheses.

These considerations explain to us why certain theories, supposed to be abandoned and finally condemned by experiment, suddenly arise from their ashes and recommence a new life. It is because they expressed true relations; and because they had not ceased to do so when, for one reason or another, we felt it necessary to enunciate the same relations in another language. So they retained a sort of latent life.

Scarcely fifteen years ago was there anything more ridiculous,

more naïvely antiquated, than Coulomb's fluids? And yet here they are reappearing under the name of *electrons*. Wherein do these permanently electrified molecules differ from Coulomb's electric molecules? It is true that in the electrons the electricity is supported by a little, a very little matter; in other words, they have a mass (and yet this is now contested); but Coulomb did not deny mass to his fluids, or, if he did, it was only with reluctance. It would be rash to affirm that the belief in electrons will not again suffer eclipse; it was none the less curious to note this unexpected resurrection.

But the most striking example is Carnot's principle. Carnot set it up starting from false hypotheses; when it was seen that heat is not indestructible, but may be transformed into work, his ideas were completely abandoned; afterwards Clausius returned to them and made them finally triumph. Carnot's theory, under its primitive form, expressed, aside from true relations, other inexact relations, *débris* of antiquated ideas; but the presence of these latter did not change the reality of the others. Clausius had only to discard them as one prunes dead branches.

The result was the second fundamental law of thermodynamics. There were always the same relations; though these relations no longer subsisted, at least in appearance, between the same objects. This was enough for the principle to retain its value. And even the reasonings of Carnot have not perished because of that; they were applied to a material tainted with error; but their form (that is to say, the essential) remained correct.

What I have just said illuminates at the same time the rôle of general principles such as the principle of least action, or that of the conservation of energy.

These principles have a very high value; they were obtained in seeking what there was in common in the enunciation of numerous physical laws; they represent therefore, as it were, the quintessence of innumerable observations.

However, from their very generality a consequence results to which I have called attention in Chapter VIII., namely, that they can no longer be verified. As we can not give a general definition of energy, the principle of the conservation of energy signifies simply that there is *something* which remains constant. Well,

whatever be the new notions that future experiments shall give us about the world, we are sure in advance that there will be something there which will remain constant and which may be called *energy*.

Is this to say that the principle has no meaning and vanishes in a tautology? Not at all; it signifies that the different things to which we give the name of *energy* are connected by a true kinship; it affirms a real relation between them. But then if this principle has a meaning, it may be false; it may be that we have not the right to extend indefinitely its applications, and yet it is certain beforehand to be verified in the strict acceptance of the term; how then shall we know when it shall have attained all the extension which can legitimately be given it? Just simply when it shall cease to be useful to us, that is, to make us correctly foresee new phenomena. We shall be sure in such a case that the relation affirmed is no longer real; for otherwise it would be fruitful; experiment, without directly contradicting a new extension of the principle, will yet have condemned it.

PHYSICS AND MECHANISM.—Most theorists have a constant predilection for explanations borrowed from mechanics or dynamics. Some would be satisfied if they could explain all phenomena by motions of molecules attracting each other according to certain laws. Others are more exacting; they would suppress attractions at a distance; their molecules should follow rectilinear paths from which they could be made to deviate only by impacts. Others again, like Hertz, suppress forces also, but suppose their molecules subjected to geometric attachments analogous, for instance, to those of our linkages; they try thus to reduce dynamics to a sort of kinematics.

In a word, all would bend nature into a certain form outside of which their mind could not feel satisfied. Will nature be sufficiently flexible for that?

We shall examine this question in Chapter XII., *à propos* of Maxwell's theory. Whenever the principles of energy and of least action are satisfied, we shall see not only that there is always one possible mechanical explanation, but that there is always an infinity of them. Thanks to a well-known theorem of Königs on linkages, it could be shown that we can, in an infinity of ways, explain

everything by attachments after the manner of Hertz, or also by central forces. Without doubt it could be demonstrated just as easily that everything can always be explained by simple impacts.

For that, of course, we need not be content with ordinary matter, with that which falls under our senses and whose motions we observe directly. Either we shall suppose that this common matter is formed of atoms whose internal motions elude us, the displacement of the totality alone remaining accessible to our senses. Or else we shall imagine some one of those subtile fluids which under the name of *ether* or under other names, have at all times played so great a rôle in physical theories.

Often one goes further and regards the ether as the sole primitive matter or even as the only true matter. The more moderate consider common matter as condensed ether, which is nothing startling; but others reduce still further its importance and see in it nothing more than the geometric locus of the ether's singularities. For instance, what we call *matter* is for Lord Kelvin only the locus of points where the ether is animated by vortex motions; for Riemann, it was the locus of points where ether is constantly destroyed; for other more recent authors, Wiechert or Larmor, it is the locus of points where the ether undergoes a sort of torsion of a very particular nature. If the attempt is made to occupy one of these points of view, I ask myself by what right shall we extend to the ether, under pretext that this is the true matter, mechanical properties observed in ordinary matter, which is only false matter.

The ancient fluids, caloric, electricity, etc., were abandoned when it was perceived that heat is not indestructible. But they were abandoned for another reason also. In materializing them, their individuality was, so to speak, emphasized, a sort of abyss was opened between them. This had to be filled up on the coming of a more vivid feeling of the unity of nature, and the perception of the intimate relations which bind together all its parts. Not only did the old physicists, in multiplying fluids, create entities unnecessarily, but they broke real ties.

It is not sufficient for a theory to affirm no false relations, it must not hide true relations.

And does our ether really exist? We know the origin of our belief in the ether. If light reaches us from a distant star, during

several years it was no longer on the star and not yet on the earth; it must then be somewhere and sustained, so to speak, by some material support.

The same idea may be expressed under a more mathematical and more abstract form. What we ascertain are the changes undergone by material molecules; we see, for instance, that our photographic plate feels the consequences of phenomena of which the incandescent mass of the star was the theater several years before. Now, in ordinary mechanics the state of the system studied depends only on its state at an instant immediately anterior; therefore the system satisfies differential equations. On the contrary, if we should not believe in the ether, the state of the material universe would depend not only on the state immediately preceding, but on states much older; the system would satisfy equations of finite differences. It is to escape this derogation of the general laws of mechanics that we have invented the ether.

That would still only oblige us to fill up, with the ether, the interplanetary void, but not to make it penetrate the bosom of the material media themselves. Fizeau's experiment goes further. By the interference of rays which have traversed air or water in motion, it seems to show us two different media interpenetrating and yet changing place one with regard to the other.

We seem to touch the ether with the finger.

Yet experiments may be conceived which would make us touch it still more nearly. Suppose Newton's principle, of the equality of action and reaction, no longer true if applied to matter *alone*, and that we have established it. The geometric sum of all the forces applied to all the material molecules would no longer be null. It would be necessary then, if we did not wish to change all mechanics, to introduce the ether, in order that this action which matter appeared to experience should be counterbalanced by the reaction of matter on something.

Or again, suppose we discover that optical and electrical phenomena are influenced by the motion of the earth. We should be led to conclude that these phenomena might reveal to us not only the relative motions of material bodies, but what would seem to be their absolute motions. Again, an ether would be necessary, that these so-called absolute motions should not be their

displacements with regard to a void space, but their displacements with regard to something concrete.

Shall we ever arrive at that? I have not this hope, I shall soon say why, and yet it is not so absurd, since others have had it.

For instance, if the theory of Lorentz, of which I shall speak in detail further on in Chapter XIII., were true, Newton's principle would not apply to matter *alone*, and the difference would not be very far from being accessible to experiment.

On the other hand, many researches have been made on the influence of the earth's motion. The results have always been negative. But these experiments were undertaken because the outcome was not sure in advance, and, indeed, according to the ruling theories, the compensation would be only approximate, and one might expect to see precise methods give positive results.

I believe that such a hope is illusory; it was none the less interesting to show that a success of this sort would open to us, in some sort, a new world.

And now I must be permitted a digression; I must explain, in fact, why I do not believe, despite Lorentz, that more precise observations can ever put in evidence anything else than the relative displacements of material bodies. Experiments have been made which should have disclosed the terms of the first order; the results have been negative; could that be by chance? No one has assumed that; a general explanation has been sought, and Lorentz has found it; he has shown that the terms of the first order must destroy each other, but not those of the second. Then more precise experiments were made; they also were negative; neither could this be the effect of chance; an explanation was necessary; it was found; they always are found; of hypotheses there is never lack.

But this is not enough; who does not feel that this is still to leave to chance too great a rôle? Would not that also be a chance, this singular coincidence which brought it about that a certain circumstance should come just in the nick of time to destroy the terms of the first order, and that another circumstance, wholly different, but just as opportune, should take upon itself to destroy those of the second order? No, it is necessary to find an explanation the same for the one as for the other, and then

everything leads us to think that this explanation will hold good equally well for the terms of higher order, and that the mutual destruction of these terms will be rigorous and absolute.

PRESENT STATE OF THE SCIENCE.—In the history of the development of physics we distinguish two inverse tendencies.

On the one hand, new bonds are continually being discovered between objects which had seemed destined to remain forever unconnected; scattered facts cease to be strangers to one another; they tend to arrange themselves in an imposing synthesis. Science advances toward unity and simplicity.

On the other hand, observation reveals to us every day new phenomena; they must long await their place and sometimes, to make one for them, a corner of the edifice must be demolished. In the known phenomena themselves, where our crude senses showed us uniformity, we perceive details from day to day more varied; what we believed simple becomes complex, and science appears to advance toward variety and complexity.

Of these two inverse tendencies, which seem to triumph turn about, which will win? If it be the first, science is possible; but nothing proves this *a priori*, and it may well be feared that after having made vain efforts to bend nature in spite of herself to our ideal of unity, submerged by the ever-rising flood of our new riches, we must renounce classifying them, abandon our ideal, and reduce science to the registration of innumerable recipes.

To this question we can not reply. All we can do is to observe the science of to-day and compare it with that of yesterday. From this examination we may doubtless draw some encouragement.

Half a century ago, hope ran high. The discovery of the conservation of energy and of its transformations had revealed to us the unity of force. Thus it showed that the phenomena of heat could be explained by molecular motions. What was the nature of these motions was not exactly known, but no one doubted that it soon would be. For light, the task seemed completely accomplished. In what concerns electricity, things were less advanced. Electricity had just annexed magnetism. This was a considerable step toward unity, and a decisive step.

But how should electricity in its turn enter into the general unity, how should it be reduced to the universal mechanism?

Of that no one had any idea. Yet the possibility of this reduction was doubted by none, there was faith. Finally, in what concerns the molecular properties of material bodies, the reduction seemed still easier, but all the detail remained hazy. In a word, the hopes were vast and animated, but vague. To-day, what do we see? First of all, a prime progress, immense progress. The relations of electricity and light are now known; the three realms, of light, of electricity and of magnetism, previously separated, form now but one; and this annexation seems final.

This conquest, however, has cost us some sacrifices. The optical phenomena subordinate themselves as particular cases under the electrical phenomena; so long as they remained isolated, it was easy to explain them by motions that were supposed to be known in all their details, that was a matter of course; but now an explanation, to be acceptable, must be easily capable of extension to the entire electric domain. Now that is a matter not without difficulties.

The most satisfactory theory we have is that of Lorentz, which, as we shall see in the last chapter, explains electric currents by the motions of little electrified particles; it is unquestionably the one which best explains the known facts, the one which illuminates the greatest number of true relations, the one of which most traces will be found in the final construction. Nevertheless it still has a serious defect, which I have indicated above; it is contrary to Newton's law of the equality of action and reaction; or rather, this principle, in the eyes of Lorentz, would not be applicable to matter alone; for it to be true, it would be necessary to take account of the action of the ether on matter and of the reaction of matter on the ether.

Now, from what we know at present, it seems probable that things do not happen in this way.

However that may be, thanks to Lorentz, Fizeau's results on the optics of moving bodies, the laws of normal and anomalous dispersion and of absorption find themselves linked to one another and to the other properties of the ether by bonds which beyond any doubt will never more be broken. See the facility with which the new Zeeman effect has found its place already and has even aided in classifying Faraday's magnetic rotation which

had defied Maxwell's efforts; this facility abundantly proves that the theory of Lorentz is not an artificial assemblage destined to fall asunder. It will probably have to be modified, but not destroyed.

But Lorentz had no aim beyond that of embracing in one totality all the optics and electrodynamics of moving bodies; he never pretended to give a mechanical explanation of them. Larmor goes further; retaining the theory of Lorentz in essentials, he grafts upon it, so to speak, MacCullagh's ideas on the direction of the motions of the ether.

According to him, the velocity of the ether would have the same direction and the same magnitude as the magnetic force. However ingenious this attempt may be, the defect of the theory of Lorentz remains and is even aggravated. With Lorentz, we do not know what are the motions of the ether; thanks to this ignorance, we may suppose them such that, compensating those of matter, they reestablish the equality of action and reaction. With Larmor, we know the motions of the ether, and we can ascertain that the compensation does not take place.

If Larmor has failed, as it seems to me he has, does that mean that a mechanical explanation is impossible? Far from it: I have said above that when a phenomenon obeys the two principles of energy and of least action, it admits of an infinity of mechanical explanations; so it is, therefore, with the optical and electrical phenomena.

But this is not enough: for a mechanical explanation to be good, it must be simple; for choosing it among all which are possible, there should be other reasons besides the necessity of making a choice. Well, we have not as yet a theory satisfying this condition and consequently good for something. Must we lament this? That would be to forget what is the goal sought; this is not mechanism; the true, the sole aim is unity.

We must therefore set bounds to our ambition; let us not try to formulate a mechanical explanation; let us be content with showing that we could always find one if we wished to. In this regard we have been successful; the principle of the conservation of energy has received only confirmations; a second principle has come to join it, that of least action, put under the form which is

suitable for physics. It also has always been verified, at least in so far as concerns reversible phenomena which thus obey the equations of Lagrange, that is to say, the most general laws of mechanics.

Irreversible phenomena are much more rebellious. Yet these also are being coordinated, and tend to come into unity; the light which has illuminated them has come to us from Carnot's principle. Long did thermodynamics confine itself to the study of the dilatation of bodies and their changes of state. For some time past it has been growing bolder and has considerably extended its domain. We owe to it the theory of the galvanic battery, and that of the thermoelectric phenomena; there is not in all physics a corner that it has not explored, and it has attacked chemistry itself.

Everywhere the same laws reign; everywhere, under the diversity of appearances, is found again Carnot's principle; everywhere also is found that concept so prodigiously abstract of entropy, which is as universal as that of energy and seems like it to cover a reality. Radiant heat seemed destined to escape it; but recently we have seen that submit to the same laws.

In this way fresh analogies are revealed to us, which may often be followed into detail; ohmic resistance resembles the viscosity of liquids; hysteresis would resemble rather the friction of solids. In all cases, friction would appear to be the type which the most various irreversible phenomena copy, and the kinship is real and profound.

Of these phenomena a mechanical explanation, properly so called, has also been sought. They hardly lent themselves to it. To find it, it was necessary to suppose that the irreversibility is only apparent, that the elementary phenomena are reversible and obey the known laws of dynamics. But the elements are extremely numerous and blend more and more, so that to our crude sight all appears to tend toward uniformity, that is, everything seems to go forward in the same sense without hope of return. The apparent irreversibility is thus only an effect of the law of great numbers. But, only a being with infinitely subtle senses, like Maxwell's imaginary demon, could disentangle this inextricable skein and turn back the course of the universe.

This conception, which attaches itself to the kinetic theory of gases, has cost great efforts and has not, on the whole, been fruitful; but it may become so. This is not the place to examine whether it does not lead to contradictions and whether it is in conformity with the true nature of things.

We signalize, however, M. Gouy's original ideas on the Brownian movement. According to this scientist, this singular motion should escape Carnot's principle. The particles which it puts in swing would be smaller than the links of that so compacted skein; they would therefore be fitted to disentangle them and hence to make the world go backward. We should almost see Maxwell's demon at work.

To summarize, the previously known phenomena are better and better classified, but new phenomena come to claim their place; most of these, like the Zeeman effect, have at once found it.

But we have the cathode rays, the X-rays, those of uranium and of radium. Herein is a whole world which no one suspected. How many unexpected guests must be stowed away!

No one can yet foresee the place they will occupy. But I do not believe they will destroy the general unity; I think they will rather complete it. On the one hand, in fact, the new radiations seem connected with the phenomena of luminescence; not only do they excite fluorescence, but they sometimes take birth in the same conditions as it.

Nor are they without kinship with the causes which produce the electric spark under the action of the ultra-violet light.

Finally, and above all, it is believed that in all these phenomena are found true ions, animated, it is true, by velocities incomparably greater than in the electrolytes.

That is all very vague, but it will all become more precise.

Phosphorescence, the action of light on the spark, these were regions rather isolated, and consequently somewhat neglected by investigators. One may now hope that a new path will be constructed which will facilitate their communications with the rest of science.

Not only do we discover new phenomena, but in those we thought we knew, unforeseen aspects reveal themselves. In the free ether, the laws retain their majestic simplicity; but matter,

properly so called, seems more and more complex; all that is said of it is never more than approximate, and at each instant our formulas require new terms.

Nevertheless the frames are not broken; the relations that we have recognized between objects we thought simple still subsist between these same objects when we know their complexity, and it is that alone which is of importance. Our equations become, it is true, more and more complicated, in order to embrace more closely the complexity of nature; but nothing is changed in the relations which permit the deducing of these equations one from another. In a word, the form of these equations has persisted.

Take, for example, the laws of reflection: Fresnel had established them by a simple and seductive theory which experiment seemed to confirm. Since then more precise researches have proved that this verification was only approximate; they have shown everywhere traces of elliptic polarization. But, thanks to the help that the first approximation gave us, we found forthwith the cause of these anomalies, which is the presence of a transition layer; and Fresnel's theory has subsisted in its essentials.

But there is a reflection we can not help making: All these relations would have remained unperceived if one had at first suspected the complexity of the objects they connect. It has long been said: If Tycho had had instruments ten times more precise, neither Kepler, nor Newton, nor astronomy would ever have been. It is a misfortune for a science to be born too late, when the means of observation have become too perfect. This is to-day the case with physical chemistry; its founders are embarrassed in their general grasp by third and fourth decimals; happily they are men of a robust faith.

The better one knows the properties of matter, the more one sees continuity reign. Since the labors of Andrews and of van der Wals, we get an idea of how the passage is made from the liquid to the gaseous state, and that this passage is not abrupt. Similarly, there is no gap between the liquid and solid states, and in the proceedings of a recent congress is to be seen alongside of a work on the rigidity of liquids, a memoir on the flow of solids.

By this tendency no doubt simplicity loses; some phenomenon was formerly represented by several straight lines, now these

straights must be joined by curves more or less complicated. In compensation unity gains notably. Those cut-off categories quieted the mind, but they did not satisfy it.

Finally the methods of physics have invaded a new domain, that of chemistry; physical chemistry is born. It is still very young, but we already see that it will enable us to connect such phenomena as electrolysis, osmosis and the motions of ions.

From this rapid exposition, what shall we conclude?

Everything considered, we have approached unity; we have not been as quick as was hoped fifty years ago, we have not always taken the predicted way; but, finally, we have gained ever so much ground.

CHAPTER XI.

THE CALCULUS OF PROBABILITIES.

DOUBTLESS it will be astonishing to find here thoughts about the calculus of probabilities. What has it to do with the method of the physical sciences? And yet the questions I shall raise without solving present themselves naturally to the philosopher who is thinking about physics. So far is this the case that in the two preceding chapters I have often been led to use the words 'probability' and 'chance.'

'Predicted facts,' as I have said above, 'can only be probable.' "However solidly founded a prediction may seem to us to be, we are never absolutely sure that experiment will not prove it false. But the probability is often so great that practically we may be satisfied with it." And a little further on I have added: "See what a rôle the belief in simplicity plays in our generalizations. We have verified a simple law in a great number of particular cases; we refuse to admit that this coincidence, so often repeated, can be a mere effect of chance. . . ."

Thus in a multitude of circumstances the physicist is in the same position as the gambler who reckons up his chances. As often as he reasons by induction, he requires more or less consciously the calculus of probabilities, and this is why I am obliged to introduce a parenthesis, and interrupt our study of method in the physical sciences in order to examine a little more closely the value of this calculus, and what confidence it merits.

The very name calculus of probabilities is a paradox. Probability opposed to certainty is what we do not know, and how can we calculate what we do not know. Yet many eminent savants have occupied themselves with this calculus, and it can not be denied that science has drawn therefrom no small advantage.

How can we explain this apparent contradiction?

Has probability been defined? Can it even be defined? And if it can not, how dare we reason about it? The definition, it will be said, is very simple: the probability of an event is the ratio of

the number of cases favorable to this event to the total number of possible cases.

A simple example will show how incomplete this definition is. I throw two dice. What is the probability that one of the two at least turns up a six? Each die can turn up in six different ways; the number of possible cases is $6 \times 6 = 36$; the number of favorable cases is 11; the probability is $11/36$.

That is the correct solution. But could I not just as well say: The points which turn up on the two dice can form $6 \times 7/2 = 21$ different combinations? Among these combinations 6 are favorable; the probability is $6/21$.

Now why is the first method of enumerating the possible cases more legitimate than the second? In any case, it is not our definition that tells us.

We are therefore obliged to complete this definition by saying: ‘. . . to the total number of possible cases provided these cases are equally probable.’ So, therefore, we are reduced to defining the probable by the probable.

How can we know that two possible cases are equally probable? Will it be by a convention? If we place at the beginning of each problem an explicit convention, well and good. We shall then have nothing to do but apply the rules of arithmetic and of algebra, and we shall complete our calculation without our result, leaving room for doubt. But if we wish to make the slightest application of this result, we must prove our convention was legitimate, and we shall find ourselves in the presence of the very difficulty we thought to escape.

Will it be said that good sense suffices to show us what convention should be adopted? Alas! M. Bertrand has amused himself by discussing the following simple problem: “What is the probability that a chord of a circle may be greater than the side of the inscribed equilateral triangle?” The illustrious geometer successively adopted two conventions which good sense seemed equally to dictate, and with one he found $1/2$, with the other $1/3$.

The conclusion which seems to follow from all this is that the calculus of probabilities is a useless science, and that the obscure instinct which we may call good sense, and to which we are wont to appeal to legitimize our conventions, must be distrusted.

But neither can we subscribe to this conclusion; we can not do without this obscure instinct. Without it science would be impossible, without it we could neither discover a law nor apply it. Have we the right, for instance, to enunciate Newton's law? Without doubt, numerous observations are in accord with it; but is not this a simple effect of chance? Besides, how do we know whether this law, true for so many centuries, will still be true next year? To this objection, you will find nothing to reply, except: 'That is very improbable.'

But grant the law. Thanks to it, I believe myself able to calculate the position of Jupiter a year from now. Have I the right to believe this? Who can tell if a gigantic mass of enormous velocity will not between now and that time pass near the solar system, and produce unforeseen perturbations? Here again the only answer is: 'It is very improbable.'

From this point of view, all the sciences would be only unconscious applications of the calculus of probabilities. To condemn this calculus would be to condemn the whole of science.

I shall dwell less on the scientific problems in which the intervention of the calculus of probabilities is more evident. In the forefront of these is the problem of interpolation, in which, knowing a certain number of values of a function, we seek to divine the intermediate values.

I shall likewise mention: the celebrated theory of errors of observation, to which I shall return later; the kinetic theory of gases, a well-known hypothesis, wherein each gaseous molecule is supposed to describe an extremely complicated trajectory; but in which, through the effect of great numbers, the mean phenomena, alone observable, obey the simple laws of Mariotte and Gay-Lussac.

All these theories are based on the laws of great numbers, and the calculus of probabilities would evidently involve them in its ruin. It is true that they have only a particular interest, and that, save as far as interpolation is concerned, these are sacrifices to which we might readily be resigned.

But, as I have said above, it would not be only these partial sacrifices that would be in question; it would be the legitimacy of the whole of science that would be challenged.

I quite see that it might be said: "We are ignorant, and yet

we must act. For action, we have not time to devote ourselves to an inquiry sufficient to dispel our ignorance. Besides, such an inquiry would demand an infinite time. We must therefore decide without knowing; we are obliged to do so, hit or miss, and we must follow rules without quite believing them. What I know is not that such and such a thing is true, but that the best course for me is to act as if it were true." The calculus of probabilities, and consequently science itself, would thenceforth have merely a practical value.

Unfortunately the difficulty does not thus disappear. A gambler wants to try a *coup*; he asks my advice. If I give it to him, I shall use the calculus of probabilities, but I shall not guarantee success. This is what I shall call *subjective probability*. In this case, we might be content with the explanation of which I have just given a sketch. But suppose that an observer is present at the game, that he notes all its *coups*, and that the game goes on a long time. When he makes a summary of his book, he will find that events have taken place in conformity with the laws of the calculus of probabilities. This is what I shall call *objective probability*, and it is this phenomenon which has to be explained.

There are numerous insurance companies which apply the rules of the calculus of probabilities, and they distribute to their shareholders dividends whose objective reality can not be contested. To invoke our ignorance and the necessity to act does not suffice to explain them.

Thus absolute skepticism is not admissible. We may distrust, but we can not condemn *en bloc*. Discussion is necessary.

I. CLASSIFICATION OF THE PROBLEMS OF PROBABILITY.—In order to classify the problems which present themselves *à propos* of probabilities, we may look at them from many different points of view, and, first, from the *point of view of generality*. I have said above that probability is the ratio of the number of favorable cases to the number of possible cases. What for want of a better term I call the generality will increase with the number of possible cases. This number may be finite, as, for instance, if we take a throw of the dice in which the number of possible cases is 36. That is the first degree of generality.

But if we ask, for example, what is the probability that a

point within a circle is within the inscribed square, there are as many possible cases as there are points in the circle, that is to say, an infinity. This is the second degree of generality. Generality can be pushed further still. We may ask the probability that a function will satisfy a given condition. There are then as many possible cases as one can imagine different functions. This is the third degree of generality, to which we rise, for instance, when we seek to find the most probable law according to a finite number of observations.

We may place ourselves at a point of view wholly different. If we were not ignorant, there would be no probability, there would be room for nothing but certainty. But our ignorance can not be absolute, for then there would no longer be any probability at all, since a little light is necessary to attain even this uncertain science. Thus the problems of probability may be classed according to the greater or less depth of this ignorance.

In mathematics even we may set ourselves problems of probability. What is the probability that the fifth decimal of a logarithm taken at random from a table is a '9'? There is no hesitation in answering that this probability is $1/10$; here we possess all the data of the problem. We can calculate our logarithm without recourse to the table, but we do not wish to give ourselves the trouble. This is the first degree of ignorance.

In the physical sciences our ignorance becomes greater. The state of a system at a given instant depends on two things: Its initial state, and the law according to which that state varies. If we know both this law and this initial state, we shall have then only a mathematical problem to solve, and we fall back upon the first degree of ignorance.

But it often happens that we know the law, and do not know the initial state. It may be asked, for instance, what is the present distribution of the minor planets? We know that from all time they have obeyed the laws of Kepler, but we do not know what was their initial distribution.

In the kinetic theory of gases, we assume that the gaseous molecules follow rectilinear trajectories, and obey the laws of impact of elastic bodies. But, as we know nothing of their initial velocities, we know nothing of their present velocities.

The calculus of probabilities only enables us to predict the mean phenomena which will result from the combination of these velocities. This is the second degree of ignorance.

Finally it is possible that not only the initial conditions but the laws themselves are unknown. We then reach the third degree of ignorance, and in general we can no longer affirm anything at all as to the probability of a phenomenon.

It often happens that instead of trying to guess an event, by means of a more or less imperfect knowledge of the law, the events may be known, and we want to find the law; or that instead of deducing effects from causes, we wish to deduce the causes from the effects. These are the problems called *probability of causes*, the most interesting from the point of view of their scientific applications.

I play *écarté* with a gentleman I know to be perfectly honest. He is about to deal. What is the probability of his turning up the king? It is $1/8$. This is a problem of the probability of effects.

I play with a gentleman whom I do not know. He has dealt ten times, and he has turned up the king six times. What is the probability that he is a sharper? This is a problem in the probability of causes.

It may be said that this is the essential problem of the experimental method. I have observed n values of x and the corresponding values of y . I have found that the ratio of the latter to the former is practically constant. There is the event, what is the cause?

Is it probable that there is a general law according to which y would be proportional to x , and that the small divergencies are due to errors of observation? This is a type of question that one is ever asking, and which we unconsciously solve whenever we are engaged in scientific work.

I am now going to pass in review these different categories of problems, discussing in succession what I have called above subjective and objective probability.

II. PROBABILITY IN MATHEMATICS.—The impossibility of squaring the circle has been proved since 1882; but even before that recent date all geometers considered that impossibility as so ‘probable,’ that the Academy of Sciences rejected without exami-

nation the alas! too numerous memoirs on this subject, that some unhappy madman sent in every year.

Was the Academy wrong? Evidently not, and it knew well that in acting thus, it did not run the least risk of stifling a discovery of moment. The Academy could not have proved that it was right; but it knew quite well that its instinct was not mistaken. If you had asked the academicians, they would have answered: "We have compared the probability that an unknown savant should have found out what has been vainly sought for so long, with the probability that there is the one madman the more on the earth; the second appears to us the greater." These are very good reasons, but there is nothing mathematical about them; they are purely psychological.

And if you had pressed them further, they would have added: "Why do you wish a particular value of a transcendental function to be an algebraic number; and if π were a root of an algebraic equation, why do you wish this root to be a period of the function $\sin 2x$, and not the same about the other roots of this same equation?" To sum up, they would have invoked the principle of sufficient reason in its vaguest form.

But what could they deduce from it? At most a rule of conduct for the employment of their time, more usefully spent at their ordinary work than in reading a lucubration that inspired in them a legitimate distrust. But what I call above objective probability has nothing in common with this first problem.

It is otherwise with the second problem.

Consider the first 10,000 logarithms that we find in a table. Among these 10,000 logarithms I take one at random. What is the probability that its third decimal is an even number? You will not hesitate to answer $1/2$; and in fact if you pick out in a table the third decimals of these 10,000 numbers, you will find nearly as many even digits as odd.

Or if you prefer, let us write 10,000 numbers corresponding to our 10,000 logarithms, each of these numbers being $+1$, if the third decimal of the corresponding logarithm is even, and -1 if odd. Then take the mean of these 10,000 numbers.

I do not hesitate to say that the mean of these 10,000 numbers is probably 0, and if I were actually to calculate it I should verify that it is extremely small.

But even this verification is needless. I might have rigorously proved that this mean is less than 0.003. To prove this result, I should have had to make a rather long calculation for which there is no room here, and for which I confine myself to citing an article I published in the *Revue générale des Sciences*, April 15, 1899. The only point to which I wish to call attention is the following: in this calculation, I should have needed only to rest my case on two facts, to wit, that the first and second derivatives of the logarithm remain in the interval considered between certain limits.

Hence this important consequence that the property is true not only of the logarithm, but of any continuous function whatever, since the derivatives of every continuous function are limited.

If I was certain beforehand of the result, it is first, because I had often observed analogous facts for other continuous functions; and next, because I made in my mind, in a more or less unconscious and imperfect manner, the reasoning which led me to the preceding inequalities, just as a skilled calculator before finishing his multiplication takes into account what it should come to approximately.

And besides, since what I call my intuition was only an incomplete summary of a piece of true reasoning, it is clear why observation has confirmed my predictions, and why the objective probability has been in agreement with the subjective probability.

As a third example I shall choose the following problem: A number u is taken at random, and n is a given very large integer. What is the probable value of $\sin nu$. This problem has no meaning by itself. To give it one a convention is needed. We shall agree that the probability for the number u to lie between a and $a + da$ is equal to $\phi(a)da$; that it is therefore proportional to the infinitely small interval da , and equal to this multiplied by a function $\phi(a)$ depending only on a . As for this function, I choose it arbitrarily, but I must assume it to be continuous. The value of $\sin nu$ remaining the same when u increases by 2π , I may without loss of generality assume that u lies between 0 and 2π , and I shall thus be led to suppose that $\phi(a)$ is a periodic function whose period is 2π .

The probable value sought is readily expressed by a simple integral, and it is easy to show that this integral is less than

$$2\pi M_k/n^k,$$

M_k being the maximum value of the k^{th} derivative of $\phi(u)$. We see then that if the k^{th} derivative is finite, our probable value will tend toward 0 when n increases indefinitely, and that more rapidly than $1/n^{k-1}$.

The probable value of $\sin nu$, when n is very large is therefore naught. To define this value I require a convention; but the result remains the same *whatever that convention may be*. I have imposed upon myself only slight restrictions in assuming that the function $\phi(a)$ is continuous and periodic, and these hypotheses are so natural that we may ask ourselves how they can be escaped.

Examination of the three preceding examples, so different in all respects, has already given us a glimpse, on the one hand, of the rôle of what philosophers call the principle of sufficient reason, and, on the other hand, of the importance of the fact that certain properties are common to all continuous functions. The study of probability in the physical sciences will lead us to the same result.

III. PROBABILITY IN THE PHYSICAL SCIENCES.—We come now to the problems connected with what I have called the second degree of ignorance, those, namely, in which we know the law, but do not know the initial state of the system. I could multiply examples, but will take only one. What is the probable present distribution of the minor planets on the zodiac?

We know they obey the laws of Kepler. We may even, without at all changing the nature of the problem, suppose that their orbits are all circular, and situated in the same plane, and that we know this. On the other hand, we are in absolute ignorance as to what was their initial distribution. However, we do not hesitate to affirm that this present distribution is now nearly uniform. Why?

Let b be the longitude of a minor planet in the initial epoch, that is to say, the epoch zero. Let a be its mean motion. Its longitude at the present epoch, that is to say, at the epoch t , will be $at + b$. To say that the present distribution is uniform is to say that the mean value of the sines and cosines of multiples of $at + b$ is zero. Why do we assert this?

Let us represent each minor planet by a point in a plane, to

wit, by a point whose coordinates are precisely a and b . All these representative points will be contained in a certain region of the plane, but as they are very numerous, this region will appear dotted with points. We know nothing else about the distribution of these points.

What do we do when we wish to apply the calculus of probabilities to such a question? What is the probability that one or more representative points may be found in a certain portion of the plane? In our ignorance, we are reduced to making an arbitrary hypothesis. To explain the nature of this hypothesis, allow me to use, in lieu of a mathematical formula, a crude but concrete image. Let us suppose that over the surface of our plane has been spread an imaginary substance, whose density is variable, but varies continuously. We shall then agree to say that the probable number of representative points to be found on a portion of the plane is proportional to the quantity of fictitious matter found there. If we have then two regions of the plane of the same extent, the probabilities that a representative point of one of our minor planets is found in one or the other of these regions will be to one another as the mean densities of the fictitious matter in the one and the other region.

Here then are two distributions, one real, in which the representative points are very numerous, very close together, but discrete like the molecules of matter in the atomic hypothesis; the other remote from reality, in which our representative points are replaced by continuous fictitious matter. We know that the latter can not be real, but our ignorance forces us to adopt it.

If again we had some idea of the real distribution of the representative points, we could arrange it so that in a region of some extent the density of this imaginary continuous matter would be nearly proportional to the number of the representative points, or, if you wish, to the number of atoms which are contained in that region. Even that is impossible, and our ignorance is so great that we are forced to choose arbitrarily the function which defines the density of our imaginary matter. Only we shall be forced to an hypothesis from which we can hardly get away, we shall suppose that this function is continuous. That is sufficient, as we shall see, to enable us to reach a conclusion.

What is at the instant t the probable distribution of the minor planets? Or rather what is the probable value of the sine of the longitude at the instant t , that is to say of $\sin (at + b)$? We made at the outset an arbitrary convention, but if we adopt it, this probable value is entirely defined. Divide the plane into elements of surface. Consider the value of $\sin (at + b)$ at the center of each of these elements; multiply this value by the surface of the element, and by the corresponding density of the imaginary matter. Take then the sum for all the elements of the plane. This sum, by definition, will be the probable mean value we seek, which will thus be expressed by a double integral. It may be thought at first that this mean value depends on the choice of the function which defines the density of the imaginary matter, and that, as this function ϕ is arbitrary, we can, according to the arbitrary choice which we make, obtain any mean value. This is not so.

A simple calculation shows that our double integral decreases very rapidly when t increases. Thus I could not quite tell what hypothesis to make as to the probability of this or that initial distribution; but whatever the hypothesis made, the result will be the same, and this gets me out of my difficulty.

Whatever be the function ϕ , the mean value tends toward zero as t increases, and as the minor planets have certainly accomplished a very great number of revolutions, I may assert that this mean value is very small.

I may choose ϕ as I wish, save always one restriction: this function must be continuous; and, in fact, from the point of view of subjective probability, the choice of a discontinuous function would have been unreasonable. For instance, what reason could I have for supposing that the initial longitude might be exactly 0° , but that it could not lie between 0° and 1° ?

But the difficulty reappears if we take the point of view of objective probability, if we pass from our imaginary distribution in which the fictitious matter was supposed continuous, to the real distribution in which our representative points form, as it were, discrete atoms.

The mean value of $\sin (at + b)$ will be represented quite simply by

$$\frac{1}{n} \sum \sin (at + b),$$

n being the number of minor planets. In lieu of a double integral referring to a continuous function, we shall have a sum of discrete terms. And yet no one will seriously doubt that this mean value is practically very small.

Our representative points being very close together, our discrete sum will in general differ very little from an integral.

An integral is the limit toward which a sum of terms tends when the number of these terms is indefinitely increased. If the terms are very numerous, the sum will differ very little from its limit, that is to say from the integral, and what I said of this latter will still be true of the sum itself.

Nevertheless, there are exceptions. If, for instance, for all the minor planets,

$$b = \frac{\pi}{2} - at'$$

the longitude for all the planets at the time t would be $\pi/2$, and the mean value would evidently be equal to unity. For this to be the case, it would be necessary that at the epoch 0, the minor planets must have all been lying on a spiral of peculiar form, with its spires very close together. Every one will admit that such an initial distribution is extremely improbable (and, even supposing it realized, the distribution would not be uniform at the present time, for example, on January 1, 1906, but it would become so a few years later).

Why then do we think this initial distribution improbable? This must be explained, because if we had no reason for rejecting as improbable this absurd hypothesis everything would break down, and we could no longer make any affirmation about the probability of this or that present distribution.

Once more we shall invoke the principle of sufficient reason to which we must always recur. We might admit that at the beginning the planets were distributed almost in a straight line. We might admit that they were irregularly distributed. But it seems to us that there is no sufficient reason for the unknown cause that gave them birth to have acted along a curve so regular and yet so complicated, which would appear to have been expressly chosen so that the present distribution would not be uniform.

IV. ROUGE ET NOIR.—The questions raised by games of

chance, such as roulette, are, fundamentally, entirely analogous to those we have just treated. For example, a wheel is partitioned into a great number of equal subdivisions, alternately red and black. A needle is whirled with force, and after having made a great number of revolutions, it stops before one of these subdivisions. The probability that this division is red is evidently $1/2$. The needle describes an angle θ , including several complete revolutions. I do not know what is the probability that the needle may be whirled with a force such that this angle should lie between θ and $\theta + d\theta$; but I can make a convention. I can suppose that this probability is $\phi(\theta)d\theta$. As for the function $\phi(\theta)$, I can choose it in an entirely arbitrary manner. There is nothing that can guide me in my choice, but I am naturally led to suppose this function continuous.

Let ϵ be the length (measured on the circumference of radius 1) of each red and black subdivision. We have to calculate the integral of $\phi(\theta)d\theta$, extending it, on the one hand, to all the red divisions, and, on the other hand, to all the black divisions, and to compare the results.

Consider an interval 2ϵ , comprising a red division and a black division which follows it. Let M and m be the greatest and least values of the function $\phi(\theta)$ in this interval. The integral extended to the red divisions will be smaller than $\Sigma M\epsilon$; the integral extended to the black divisions will be greater than $\Sigma m\epsilon$; the difference will therefore be less than $\Sigma(M - m)\epsilon$. But if the function θ is supposed continuous; if, on the other hand, the interval ϵ is very small with respect to the total angle described by the needle, the difference $M - m$ will be very small. The difference of the two integrals will therefore be very small, and the probability will be very nearly $1/2$.

We see that without knowing anything of the function θ , I must act as if the probability were $1/2$. We understand, on the other hand, why, if, placing myself at the objective point of view, I observe a certain number of coups, observation will give me about as many black coups as red.

All players know this objective law; but it leads them into a remarkable error, which has been often exposed, but into which they always fall again. When the red has won, for instance, six

times running, they bet on the black, thinking they are playing a safe game; because, say they, it is very rare that red wins seven times running.

In reality their probability of winning remains $1/2$. Observation shows, it is true, that series of seven consecutive reds are very rare, but series of six reds followed by a black are just as rare.

They have noticed the rarity of the series of seven reds; if they have not remarked the rarity of six reds and a black, it is only because such series strike the attention less.

V. THE PROBABILITY OF CAUSES.—We now come to the problems of the probability of causes, the most important from the point of view of scientific applications. Two stars, for instance, are very close together on the celestial sphere. Is this apparent contiguity a mere effect of chance? Are these stars, although on almost the same visual ray, situated at very different distances from the earth, and consequently very far from one another? Or, perhaps, does the apparent correspond to a real contiguity? This is a problem on the probability of causes.

I recall first that at the outset of all problems of the probability of effects that have hitherto occupied us, we have always had to make a convention, more or less justified. And if in most cases the result was, in a certain measure, independent of this convention, this was only because of certain hypotheses which permitted us to reject *a priori* discontinuous functions, for example, or certain absurd conventions.

We shall find something analogous when we deal with the probability of causes. An effect may be produced by the cause *A* or by the cause *B*. The effect has just been observed. We ask the probability that it is due to the cause *A*. This is an *a posteriori* probability of cause. But I could not calculate it, if a convention more or less justified did not tell me *in advance* what is the *a priori* probability for the cause *A* to come into play; I mean the probability of this event for some one who had not observed the effect.

The better to explain myself I go back to the example of the game of écarté mentioned above. My adversary deals for the first time and he turns up a king. What is the probability that he is a sharper? The formulas ordinarily taught give $8/9$, a result

evidently rather surprising. If we look at it closer, we see that the calculation is made as if, *before sitting down at the table*, I had considered that there was one chance in two that my adversary was not honest. An absurd hypothesis, because in that case I should have certainly not played with him, and this explains the absurdity of the conclusion.

The convention about the *a priori* probability was unjustified, and that is why the calculation of the *a posteriori* probability led me to an inadmissible result. We see the importance of this preliminary convention. I shall even add that if none were made, the problem of the *a posteriori* probability would have no meaning. It must always be made either explicitly or tacitly.

Pass to an example of a more scientific character. I wish to determine an experimental law. This law, when I know it, can be represented by a curve. I make a certain number of isolated observations; each of these will be represented by a point. When I have obtained these different points, I draw a curve between them, striving to pass as near to them as possible, and yet preserve for my curve a regular form, without angular points, or inflections too accentuated, or brusque variation of the radius of curvature. This curve will represent for me the probable law, and I assume not only that it will tell me the values of the function intermediate between those which have been observed, but also that it will give me the observed values themselves more exactly than direct observation. This is why I make it pass near the points, and not through the points themselves.

Here is a problem in the probability of causes. The effects are the measurements I have recorded; they depend on a combination of two causes: the true law of the phenomenon and the errors of observation. Knowing the effects, we have to seek the probability that the phenomenon obeys this law or that, and that the observations have been affected by this or that error. The most probable law then corresponds to the curve traced, and the most probable error of an observation is represented by the distance of the corresponding point from this curve.

But the problem would have no meaning if, before any observation, I had not fashioned an *a priori* idea of the probability of this or that law, and of the chances of error to which I am exposed.

If my instruments are good (and that I knew before making the observations), I shall not permit my curve to depart much from the points which represent the rough measurements. If they are bad, I may go a little farther away from them in order to obtain a less sinuous curve; I shall sacrifice more to regularity.

Why then is it that I seek to trace a curve without sinuosities? It is because I consider *a priori* a law represented by a continuous function (or by a function whose derivatives of high order are small), as more probable than a law not satisfying these conditions. Without this belief, the problem of which we speak would have no meaning; interpolation would be impossible; no law could be deduced from a finite number of observations; science would not exist.

Fifty years ago physicists considered, other things being equal, a simple law as more probable than a complicated law. They even invoked this principle in favor of Mariotte's law as against the experiments of Regnault. To-day they have repudiated this belief; and yet, how many times are they compelled to act as though they still held it! However that may be, what remains of this tendency is the belief in continuity, and we have just seen that if this belief were to disappear in its turn, experimental science would become impossible.

VI. THE THEORY OF ERRORS.—We are thus led to speak of the theory of errors, which is directly connected with the problem of the probability of causes. Here again we find effects, to wit, a certain number of discordant observations, and we seek to divine the *causes*, which are, on the one hand, the real value of the quantity to be measured; on the other hand, the error made in each isolated observation. It is necessary to calculate *a posteriori* the probable magnitude of each error, and consequently the probable value of the quantity to be measured.

But as I have just explained, we should not know how to undertake this calculation if we did not admit *a priori*, that is to say, before all observation, a law of probability of errors. Is there a law of errors?

The law of errors admitted by all calculators is Gauss's law, which is represented by a certain transcendental curve known under the name of 'the bell.'

But first it is proper to recall the classic distinction between systematic and accidental errors. If we measure a length with too long a meter, we shall always find too small a number, and it will be of no use to measure several times; this is a systematic error. If we measure with an accurate meter, we may, however, make a mistake; but we go wrong, now too much, now too little, and when we take the mean of a great number of measurements, the error will tend to grow small. These are accidental errors.

It is evident from the first that systematic errors can not satisfy Gauss's law; but do the accidental errors satisfy it? A great number of demonstrations have been attempted; almost all are crude paralogisms. Nevertheless, we may demonstrate Gauss's law by starting from the following hypotheses: the error committed is the result of a great number of partial and independent errors; each of the partial errors is very little and besides, obeys any law of probability, provided that the probability of a positive error is the same as that of an equal negative error. It is evident that these conditions will be often but not always fulfilled, and we may reserve the name of accidental for errors which satisfy them.

We see that the method of least squares is not legitimate in every case; in general the physicists are more distrustful of it than the astronomers. This is, no doubt, because the latter, besides the systematic errors to which they and the physicists are subject alike, have to contend with an extremely important source of error which is wholly accidental; I mean atmospheric undulations. So it is very curious to hear a physicist discuss with an astronomer about a method of observation. The physicist, persuaded that one good measurement is worth more than many bad ones, is before all concerned with eliminating by dint of precautions the least systematic errors, and the astronomer says to him: 'But thus you can observe only a small number of stars; the accidental errors will not appear.'

What should we conclude? Must we continue to use the method of least squares? We must distinguish. We have eliminated all the systematic errors we could suspect; we know well there are still others, but we can not detect them; yet it is necessary to make up our mind and adopt a definitive value which will be

regarded as the probable value; and for that it is evident the best thing to do is to apply Gauss's method. We have only applied a practical rule referring to subjective probability. There is nothing more to be said.

But we wish to go farther and affirm that not only is the probable value so much, but that the probable error in the result is so much. *This is absolutely illegitimate*; it would be true only if we were sure that all the systematic errors were eliminated, and of that we know absolutely nothing. We have two series of observations; by applying the rule of least squares, we find that the probable error in the first series is twice as small as in the second. The second series may, however, be better than the first, because the first perhaps is affected by a large systematic error. All we can say is that the first series is *probably* better than the second, since its accidental error is smaller, and we have no reason to affirm that the systematic error is greater for one of the series than for the other, our ignorance on this point being absolute.

VII. CONCLUSIONS.—In the lines which precede, I have set many problems without solving any of them. Yet I do not regret having written them, because they will perhaps invite the reader to reflect on these delicate questions.

However that may be, there are certain points which seem well established. To undertake any calculation of probability, and even for that calculation to have any meaning, it is necessary to admit, as point of departure, an hypothesis or convention which has always something arbitrary about it. In the choice of this convention, we can be guided only by the principle of sufficient reason. Unfortunately this principle is very vague and very elastic, and in the cursory examination we have just made, we have seen it take many different forms. The form under which we have met it most often is the belief in continuity, a belief which it would be difficult to justify by apodictic reasoning, but without which all science would be impossible. Finally the problems to which the calculus of probabilities may be applied with profit are those in which the result is independent of the hypothesis made at the outset, provided only that this hypothesis satisfies the condition of continuity.

CHAPTER XII.

OPTICS AND ELECTRICITY.

FRESNEL'S THEORY.—The best example* that can be chosen of physics in the making is the theory of light and its relations to the theory of electricity. Thanks to Fresnel, optics is the best developed part of physics; the so-called wave-theory forms a whole truly satisfying to the mind. We must not, however, ask of it what it can not give us.

The object of mathematical theories is not to reveal to us the true nature of things; this would be an unreasonable pretension. Their sole aim is to coordinate the physical laws which experiment reveals to us, but which, without the help of mathematics, we should not be able even to state.

It matters little whether the ether really exists; that is the affair of metaphysicians. The essential thing for us is that everything happens as if it existed, and that this hypothesis is convenient for the explanation of phenomena. After all, have we any other reason to believe in the existence of material objects? That, too, is only a convenient hypothesis; only this will never cease to be so, whereas, no doubt, some day the ether will be thrown aside as useless. But even at that day, the laws of optics and the equations which translate them analytically will remain true, at least as a first approximation. It will always be useful, then, to study a doctrine that unites all these equations.

The undulatory theory rests on a molecular hypothesis. For those who think they have thus discovered the cause under the law, this is an advantage. For the others it is a reason for distrust. But this distrust seems to me as little justified as the illusion of the former.

These hypotheses play only a secondary part. They might be

* This chapter is a partial reproduction of the prefaces of two of my works; 'Théorie mathématique de la lumière' (Paris, Naud, 1889), and 'Électricité et optique' (Paris, Naud, 1901).

sacrificed. They usually are not, because then the explanation would lose in clearness; but that is the only reason.

In fact, if we looked closer we should see that only two things are borrowed from the molecular hypotheses: the principle of the conservation of energy, and the linear form of the equations, which is the general law of small movements, as of all small variations.

This explains why most of Fresnel's conclusions remain unchanged when we adopt the electromagnetic theory of light.

MAXWELL'S THEORY.—Maxwell, we know, connected by a close bond two parts of physics until then entirely foreign to one another, optics and electricity. By blending thus in a vaster whole, in a higher harmony, the optics of Fresnel has not ceased to be alive. Its various parts subsist, and their mutual relations are still the same. Only the language we used to express them has changed; and, on the other hand, Maxwell has revealed to us other relations, before unsuspected, between the different parts of optics and the domain of electricity.

When a French reader first opens Maxwell's book, a feeling of uneasiness and often even of mistrust mingles at first with his admiration. Only after a prolonged acquaintance and at the cost of many efforts does this feeling disappear. There are even some eminent minds that never lose it.

Why are the English scientist's ideas with such difficulty acclimatized among us? It is, no doubt, because the education received by the majority of enlightened Frenchmen predisposes them to appreciate precision and logic above every other quality.

The old theories of mathematical physics gave us in this respect complete satisfaction. All our masters, from Laplace to Cauchy, have proceeded in the same way. Starting from clearly stated hypotheses, they deduced all their consequences with mathematical rigor, and then compared them with experiment. It seemed their aim to give every branch of physics the same precision as celestial mechanics.

A mind accustomed to admire such models is hard to suit with a theory. Not only will it not tolerate the least appearance of contradiction, but it will demand that the various parts be logically connected with one another, and that the number of distinct hypotheses be reduced to minimum.

This is not all; it will have still other demands, which seem to me less reasonable. Behind the matter which our senses can reach, and which experiment tells us of, it will desire to see another, and in its eyes the only real, matter, which will have only purely geometric properties, and whose atoms will be nothing but mathematical points, subject to the laws of dynamics alone. And yet these atoms, invisible and without color, it will seek by an unconscious contradiction to represent to itself and consequently to identify as closely as possible with common matter.

Then only will it be fully satisfied and imagine that it has penetrated the secret of the universe. If this satisfaction is deceitful, it is none the less difficult to renounce.

Thus, on opening Maxwell, a Frenchman expects to find a theoretical whole as logical and precise as the physical optics based on the hypothesis of the ether; he thus prepares for himself a disappointment which I should like to spare the reader by informing him immediately of what he must look for in Maxwell, and what he can not find there.

Maxwell does not give a mechanical explanation of electricity and magnetism; he confines himself to demonstrating that such an explanation is possible.

He shows also that optical phenomena are only a special case of electromagnetic phenomena. From every theory of electricity, one can therefore deduce immediately a theory of light.

The converse unfortunately is not true; from a complete explanation of light, it is not always easy to derive a complete explanation of electric phenomena. This is not easy, in particular, if we wish to start from Fresnel's theory. Doubtless it would not be impossible; but nevertheless we must ask whether we are not going to be forced to renounce admirable results that we thought definitely acquired. That seems a step backward; and many good minds are not willing to submit to it.

When the reader shall have consented to limit his hopes, he will still encounter other difficulties. The English scientist does not try to construct a single edifice, final and well ordered; he seems rather to erect a great number of provisional and independent constructions, between which communication is difficult and sometimes impossible.

Take as example the chapter in which he explains electrostatic attractions by pressures and tensions in the dielectric medium. This chapter might be omitted without making thereby the rest of the book less clear or complete; and, on the other hand, it contains a theory complete in itself which one could understand without having read a single line that precedes or follows. But it is not only independent of the rest of the work; it is difficult to reconcile with the fundamental ideas of the book. Maxwell does not even attempt this reconciliation; he merely says: "I have not been able to make the next step, namely, to account by mechanical considerations for these stresses in the dielectric."

This example will suffice to make my thought understood; I could cite many others. Thus who would suspect, in reading the pages devoted to magnetic rotary polarization, that there is an identity between optical and magnetic phenomena?

One must not then flatter himself that he can avoid all contradiction; to that it is necessary to be resigned. In fact, two contradictory theories, provided one does not mingle them, and if one does not seek in them the basis of things, may both be useful instruments of research; and perhaps the reading of Maxwell would be less suggestive if he had not opened up to us so many new and divergent paths.

The fundamental idea, however, is thus a little obscured. So far is this the case that in the majority of popularized versions it is the only point completely left aside.

I feel, then, that the better to make its importance stand out, I ought to explain in what this fundamental idea consists. But for that a short digression is necessary.

THE MECHANICAL EXPLANATION OF PHYSICAL PHENOMENA.
—There is in every physical phenomenon a certain number of parameters which experiment reaches directly and allows us to measure. I shall call these the parameters q .

Observation then teaches us the laws of the variations of these parameters; and these laws can generally be put in the form of differential equations, which connect the parameters q with the time.

What is it necessary to do to give a mechanical interpretation of such a phenomenon?

One will try to explain it either by the motions of ordinary matter, or by those of one or more hypothetical fluids.

These fluids will be considered as formed of a very great number of isolated molecules m .

When shall we say, then, that we have a complete mechanical explanation of the phenomenon? It will be, on the one hand, when we know the differential equations satisfied by the coordinates of these hypothetical molecules m , equations which, moreover, must conform to the principles of dynamics; and, on the other hand, when we know the relations that define the coordinates of the molecules m as functions of the parameters q accessible to experiment.

These equations, as I have said, must conform to the principles of dynamics, and, in particular, to the principle of the conservation of energy and the principle of least action.

The first of these two principles teaches us that the total energy is constant and that this energy is divided into two parts:

1° The kinetic energy, or *vis viva*, which depends on the masses of the hypothetical molecules m , and their velocities, and which I shall call T .

2° The potential energy, which depends only on the coordinates of these molecules and which I shall call U . It is the *sum* of the two energies T and U which is constant.

What now does the principle of least action tell us? It tells us that to pass from the initial position occupied at the instant t_0 to the final position occupied at the instant t_1 , the system must take such a path that, in the interval of time that elapses between the two instants t_0 and t_1 , the average value of 'the action' (that is to say of the *difference* between the two energies T and U) shall be as small as possible.

If the two functions T and U are known, this principle suffices to determine the equations of motion.

Among all the possible ways of passing from one position to another, there is evidently one for which the average value of the action is less than for any other. There is, moreover, only one; and it results from this that the principle of least action suffices to determine the path followed and consequently the equations of motion.

Thus we obtain what are called the equations of Lagrange.

In these equations, the independent variables are the coordinates of the hypothetical molecules m ; but I now suppose that one takes as variables the parameters q directly accessible to experiment.

The two parts of the energy must then be expressed as functions of the parameters q and of their derivatives. They will evidently appear under this form to the experimenter. The latter will naturally try to define the potential and the kinetic energy by the aid of quantities that he can directly observe.*

That granted, the system will always go from one position to another by a path such that the average action shall be a minimum.

It matters little that T and U are now expressed by the aid of the parameters q and their derivatives; it matters little that it is also by means of these parameters that we define the initial and final positions; the principle of least action remains always true.

Now here again, of all the paths that lead from one position to another, there is one for which the average action is a minimum, and there is only one. The principle of least action suffices, then, to determine the differential equations which define the variations of the parameters q .

The equations thus obtained are another form of the equations of Lagrange.

To form these equations we need to know neither the relations that connect the parameters q with the coordinates of the hypothetical molecules, nor the masses of these molecules, nor the expression of U as a function of the coordinates of these molecules.

All we need to know is the expression of U as a function of the parameters, and that of T as a function of the parameters q and their derivatives, that is, the expressions of the kinetic and of the potential energy as functions of the experimental data.

Then we shall have one of two things: either for a suitable choice of the functions T and U , the equations of Lagrange, constructed as we have just said, will be identical with the differ-

* We add that U will depend only on the parameters q , that T will depend on the parameters q and their derivatives with respect to the time and will be a homogeneous polynomial of the second degree with respect to these derivatives.

ential equations deduced from experiments; or else there will exist no functions T and U , for which this agreement takes place. In the latter case it is clear that no mechanical explanation is possible.

The *necessary* condition for a mechanical explanation to be possible is therefore that we can choose the functions T and U in such a way as to satisfy the principle of least action, which involves that of the conservation of energy.

This condition, moreover, is *sufficient*. Suppose, in fact, that we have found a function U of the parameters q , which represents one of the parts of the energy; that another part of the energy, which we shall represent by T , is a function of the parameters q and their derivatives, and that it is a homogeneous polynomial of the second degree with respect to these derivatives; and finally that the equations of Lagrange, formed by means of these two functions, T and U , conform to the data of the experiment.

What is necessary in order to deduce from this a mechanical explanation? It is necessary that U can be regarded as the potential energy of a system and T as the *vis viva* of the same system.

There is no difficulty as to U , but can T be regarded as the *vis viva* of a material system?

It is easy to show that this is always possible, and even in an infinity of ways. I will confine myself to referring for more details to the preface of my work, 'Electricité et optique.'

Thus if the principle of least action can not be satisfied, no mechanical explanation is possible; if it can be satisfied, there is not only one, but an infinity, whence it follows that as soon as there is one there is an infinity of others.

One more observation.

Among the quantities that experiment gives us directly, we shall regard some as functions of the coordinates of our hypothetical molecules; these are our parameters q . We shall look upon the others as dependent not only on the coordinates, but on the velocities, or, what comes to the same thing, on the derivatives of the parameters q , or as combinations of these parameters and their derivatives.

And then a question presents itself: among all these quantities

measured experimentally, which shall we choose to represent the parameters q ? Which shall we prefer to regard as the derivatives of these parameters? This choice remains arbitrary in a great measure; but, for a mechanical explanation to be possible, it suffices if we can make the choice in such a way as to accord with the principle of least action.

And then Maxwell asked himself whether he could make this choice and that of the two energies T and U , in such a way that the electrical phenomena would satisfy this principle. Experiment shows us that the energy of an electromagnetic field is decomposed into two parts, the electrostatic energy and the electrodynamic energy. Maxwell observed that if we regard the first as representing the potential energy U , the second as representing the kinetic energy T ; if, moreover, the electrostatic charges of the conductors are considered as parameters q , under these conditions, I say, Maxwell observed that the electric phenomena satisfy the principle of least action. Thenceforth he was certain of the possibility of a mechanical explanation.

If he had explained this idea at the beginning of his book instead of relegating it to an obscure part of the second volume, it would not have escaped the majority of readers.

If, then, a phenomenon admits of a complete mechanical explanation, it will admit of an infinity of others, that will render an account equally well of all the particulars revealed by experiment.

And this is confirmed by the history of every branch of physics; in optics, for instance, Fresnel believed vibration to be perpendicular to the plane of polarization; Neumann regarded it as parallel to this plane. An 'experimentum crucis' has long been sought which would enable us to decide between these two theories, but it has not been found.

In the same way, without leaving the domain of electricity, we may ascertain that the theory of two fluids and that of the single fluid both account in a fashion equally satisfactory for all the observed laws of electrostatics.

All these facts are easily explicable, thanks to the properties of the equations of Lagrange which I have just recalled.

It is easy now to comprehend what is Maxwell's fundamental idea.

To demonstrate the possibility of a mechanical explanation of electricity, we need not preoccupy ourselves with finding this explanation itself; it suffices us to know the expression of the two functions T and U , which are the two parts of energy, to form with these two functions the equations of Lagrange and then to compare these equations with the experimental laws.

Among all these possible explanations, how make a choice for which the aid of experiment fails us? A day will come perhaps when physicists will not interest themselves in these questions, inaccessible to positive methods, and will abandon them to the metaphysicians. This day has not yet arrived; man does not resign himself so easily to be forever ignorant of the foundation of things.

Our choice can therefore be further guided only by considerations where the part of personal appreciation is very great; there are, however, solutions that all the world will reject because of their whimsicality, and others that all the world will prefer because of their simplicity.

In what concerns electricity and magnetism, Maxwell abstains from making any choice. It is not that he systematically disdains all that is unattainable by positive methods; the time he has devoted to the kinetic theory of gases sufficiently proves that. I will add that if, in his great work, he develops no complete explanation, he had previously attempted to give one in an article in the *Philosophical Magazine*. The strangeness and the complexity of the hypotheses he had been obliged to make had led him afterwards to give this up.

The same spirit is found throughout the whole work. What is essential, that is to say what must remain common to all theories, is made prominent; all that would only be suitable to a particular theory is nearly always passed over in silence. Thus the reader finds himself in the presence of a form almost devoid of matter, which he is at first tempted to take for a fugitive shadow not to be grasped. But the efforts to which he is thus condemned force him to think and he ends by comprehending what was often rather artificial in the theoretic constructs he had previously only wondered at.

CHAPTER XIII.

ELECTRODYNAMICS.

THE history of electrodynamics is particularly instructive from our point of view.

Ampère entitled his immortal work, 'Théorie des phénomènes électrodynamiques, *uniquement* fondée sur l'expérience.' He therefore imagined that he had made no hypothesis, but he had made them, as we shall soon see; only he made them without being conscious of it.

His successors, on the other hand, perceived them, since their attention was attracted by the weak points in Ampère's solution. They made new hypotheses, of which this time they were fully conscious; but how many times it was necessary to change them before arriving at the classic system of to-day which is perhaps not yet final; this we shall see.

I. AMPÈRE'S THEORY.—When Ampère studied experimentally the mutual actions of currents he operated and he only could operate with closed currents.

It was not that he denied the possibility of open currents. If two conductors are charged with positive and negative electricity and brought into communication by a wire, a current is established going from one to another, which continues until the two potentials are equal. According to the ideas of Ampère's time this was an open current; the current was known to go from the first conductor to the second, it was not seen to return from the second to the first.

So Ampère considered as open currents of this nature, for example, the currents of discharge of condensers; but he could not make them the objects of his experiments because their duration is too short.

Another sort of open current may also be imagined. I suppose two conductors, *A* and *B*, connected by a wire *AMB*. Small conducting masses in motion are put first in contact with the conductor *B*, take from it an electric charge, leave contact with *B* and

put themselves in motion following the path BNA , and transporting with them their charge, come into contact with A and give to it their charge, which returns then to B along the wire AMB .

Now there we have in a sense a closed circuit, since the electricity describes the closed circuit $BNAMB$; but the two parts of this current are very different. In the wire AMB , the electricity is displaced through a fixed conductor, like a voltaic current, overcoming an ohmic resistance and developing heat; we say that it is displaced by conduction. In the part BNA , the electricity is carried by a moving conductor; it is said to be displaced by convection.

If then the current of convection is considered as altogether analogous to the current of conduction, the circuit $BNAMB$ is closed; if, on the contrary, the convection current is not 'a true current,' and, for example, does not act on the magnet, there remains only the conduction current AMB , which is open.

For example, if we connect by a wire the two poles of a Holtz machine, the charged rotating disc transfers the electricity by convection from one pole to the other, and it returns to the first pole by conduction through the wire.

But currents of this sort are very difficult to produce with appreciable intensity. With the means at Ampère's disposal, we may say that this was impossible.

To sum up, Ampère could conceive of the existence of two kinds of open currents, but he could operate on neither because they were not strong enough or because their duration was too short.

Experiment therefore could only show him the action of a closed current on a closed current, or, more accurately, the action of a closed current on a portion of a current, because a current can be made to describe a closed circuit composed of a moving part and a fixed part. It is possible then to study the displacements of the moving part under the action of another closed current.

On the other hand, Ampère had no means of studying the action of an open current, either on a closed current or another open current.

1. *The Case of Closed Currents.*—In the case of the mutual action of two closed currents, experiment revealed to Ampère remarkably simple laws.

I recall rapidly here those which will be useful to us in the sequel:

1° *If the intensity of the currents is kept constant*, and if the two circuits, after having undergone any deformations and displacements whatsoever, return finally to their initial positions, the total work of the electrodynamic actions will be null.

In other words, there is an *electrodynamic potential* of the two circuits, proportional to the product of the intensities, and depending on the form and relative position of the circuits; the work of the electrodynamic actions is equal to the variation of this potential:

2° The action of a closed solenoid is null.

3° The action of a circuit C on another voltaic circuit C' depends only on the 'magnetic field' developed by this circuit. At each point in space we can in fact define in magnitude and direction a certain force called *magnetic force*, which enjoys the following properties:

(a) The force exercised by C on a magnetic pole is applied to that pole and is equal to the magnetic force multiplied by the magnetic mass of that pole;

(b) A very short magnetic needle tends to take the direction of the magnetic force, and the couple to which it tends to reduce is proportional to the magnetic force, the magnetic moment of the needle and the sine of the dip of the needle;

(c) If the circuit C is displaced, the work of the electrodynamic action exercised by C on C' will be equal to the increment of the 'flow of magnetic force' which passes through the circuit.

2. *Action of a Closed Current on a Portion of Current.*—Ampère not having been able to produce an open current, properly so called, had only one way of studying the action of a closed current on a portion of current.

This was by operating on a circuit C composed of two parts, the one fixed, the other movable. The movable part was, for instance, a movable wire $a\beta$ whose extremities a and β could slide along a fixed wire. In one of the positions of the movable wire, the end a rested on the A of the fixed wire and the extremity β on the point B of the fixed wire. The current circulated from a to β , that is to say, from A to B along the movable wire, and then it

returned from B to A along the fixed wire. *This current was therefore closed.*

In a second position, the movable wire having slipped, the extremity α rested on another point A' of the fixed wire, and the extremity β on another point B' of the fixed wire. The current circulated then from α to β , that is to say from A' to B' along the movable wire, and it afterwards returned from B to A , always following the fixed wire. The current was therefore also closed.

If a like current is subjected to the action of a closed current C , the movable part will be displaced just as if it were acted upon by a force. Ampère assumes that the apparent force to which this movable part AB seems thus subjected, representing the action of the C on the portion $\alpha\beta$ of the current, is the same as if $\alpha\beta$ were traversed by an open current, stopping at α and β , in place of being traversed by a closed current which after arriving at β returns to α through the fixed part of the circuit.

This hypothesis seems natural enough, and Ampère made it unconsciously; nevertheless *it is not necessary*, since we shall see further on that Helmholtz rejected it. However that may be, it permitted Ampère, though he had never been able to produce an open current, to enunciate the laws of the action of a closed current on an open current, or even on an element of current.

The laws are simple:

1° The force which acts on an element of current is applied to this element; it is normal to the element and to the magnetic force, and proportional to that component of that magnetic force which is normal to the element.

2° The action of a closed solenoid on an element of current is null.

But the electrodynamic potential has disappeared, that is to say, that when a closed current and an open current, whose intensities have been maintained constant, return to their initial positions, the total work is not null.

3. *Continuous Rotations.*—Among electrodynamic experiments the most remarkable are those in which continuous rotations are produced and which are sometimes called unipolar induction experiments. A magnet may turn about its axis; a current passes first through a fixed wire, enters the magnet by the pole N , for ex-

ample, passes through half the magnet, emerges by a sliding contact and reenters the fixed wire.

The magnet then begins to rotate continuously without being able ever to attain equilibrium; this is Faraday's experiment.

How is it possible? If it were a question of two circuits of invariable form, the one C fixed, the other C' movable, about an axis, this latter could never take on continuous rotation; in fact, there is an electrodynamic potential; there must therefore be necessarily a position of equilibrium when this potential is a maximum.

Continuous rotations are therefore possible only when the circuit C' is composed of two parts: one fixed, the other movable about an axis, as is the case in Faraday's experiment. Here again it is convenient to draw a distinction. The passage from the fixed to the immovable part or inversely may take place either by simple contact (the same point of the movable part remaining constantly in contact with the same point of the fixed part), or by a sliding contact (the same point of the movable part coming successively in contact with diverse points of the fixed part).

It is only in the second case that there can be continuous rotation. This is what then happens: The system tends to take a position of equilibrium; but, when at the point of reaching that position, the sliding contact puts the movable part in communication with the new point of the fixed part, it changes the connections, it changes therefore the conditions of equilibrium, so that the position of equilibrium fleeing, so to say, before the system which seeks to attain it, rotation may take place indefinitely.

Ampère assumes that the action of the circuit on the movable part of C' is the same as if the fixed part of C' did not exist, and therefore as if the current passing through a movable part were open.

He concludes therefore that the action of a closed on an open circuit, or inversely that of an open current on a closed current, may give rise to a continuous rotation.

But this conclusion depends on the hypothesis I have enunciated and which, as I said above, is not admitted by Helmholtz.

4. *Mutual Action of Two Open Currents.*—In what concerns the mutual actions of two open currents, and in particular that of

two elements of currents, all experiment breaks down. Ampère has recourse to hypothesis. He supposes:

1° That the mutual action of two elements reduces to a force acting along their join;

2° That the action of two closed currents is the resultant of the mutual action of their diverse elements, which are besides the same as if these elements were isolated.

What is remarkable is that here again Ampère makes these hypotheses unconsciously.

However that may be, these two hypotheses, together with the experiments on closed currents, suffice to determine completely the law of the mutual action of two elements. But then most of the simple laws we have met in the case of closed currents are no longer true.

In the first place, there is no electrodynamic potential; nor was there any, as we have seen, in the case of a closed current acting on an open current.

Next there is, properly speaking, no magnetic force.

And, in fact, we have given above three different definitions of this force:

1° By the action on a magnetic pole;

2° By the director couple which orientates the magnetic needle;

3° By the action on an element of current.

But in the case which now occupies us, not only these three definitions are no longer in harmony, but each has lost its meaning, and in fact:

1° A magnetic pole is no longer acted upon simply by a single force applied to this pole. We have seen in fact that the force due to the action of an element of current on a pole is not applied to the pole, but to the element; it may moreover be replaced by a force applied to the pole and to a couple;

2° The couple which acts on the magnetic needle is no longer a simple director couple, for its moment with respect to the axis of the needle is not null. It breaks up into a director couple, properly so called, and a supplementary couple which tends to produce the continuous rotation of which we have above spoken;

3° Finally the force acting on an element of current is not normal to this element.

In other words, *the unity of the magnetic force has disappeared.*

Let us see in what this unity consists. Two systems which exercise the same action on a magnetic pole, will exert also the same action on an indefinitely small magnetic needle, or on an element of current placed at the same point of space at which the pole is.

Well, this is true if these two systems contain only closed currents; this would no longer be true if these two systems contained open currents.

It suffices to remark, for instance, that if a magnetic pole is placed at *A* and an element at *B*, the direction of the element being on the prolongation of the sect *AB*, this element which will exercise no action on this pole will, on the other hand, exercise an action either on a magnetic needle placed at the point *A*, or on an element of current placed at the point *A*.

5. *Induction.*—We know that the discovery of electrodynamic induction soon followed the immortal work of Ampère.

As long as it is only a question of closed currents there is no difficulty, and Helmholtz has even remarked that the principle of the conservation of energy is sufficient for deducing the laws of induction from the electrodynamic laws of Ampère. But always on one condition, as Bertrand has well shown; that we make besides a certain number of hypotheses.

The same principle again permits this deduction in the case of open currents, although of course we can not submit the result to the test of experiment, since we can not produce such currents.

If we try to apply this mode of analysis to Ampère's theory of open currents, we reach results calculated to surprise us.

In the first place, induction can not be deduced from the variation of the magnetic field by the formula well known to savants and practitioners, and, in fact, as we have said, properly speaking there is no longer a magnetic field.

But, further; if a circuit *C* is subjected to the induction of a variable voltaic system *S*, if this system *S* be displaced and deformed in any way whatever, so that the intensity of the currents of this system varies according to any law whatever, but that after these variations the system finally returns to its initial situation, it

seems natural to suppose that the *mean* electromotive force induced in the circuit C is null.

This is true if the circuit C is closed and if the system S contains only closed currents. This would no longer be true, if one accepts the theory of Ampère, if there were open currents. So that not only induction will no longer be the variation of the flow of magnetic force, in any of the usual senses of the word, but it can not be represented by the variation of that force, whatever it may be.

II. THEORY OF HELMHOLTZ.—I have dwelt upon the consequences of Ampère's theory, and of his method of explaining open currents.

It is difficult to overlook the paradoxical and artificial character of the propositions to which we are thus led. One can not help thinking 'that can not be so.'

We understand therefore why Helmholtz was led to seek something else.

Helmholtz rejects Ampère's fundamental hypothesis, to wit, that the mutual action of two elements of currents reduces to a force along their join. He assumes that an element of current is not subjected to a single force, but to a force and a couple. It is just this which gave rise to the celebrated polemic between Bertrand and Helmholtz.

Helmholtz replaces Ampère's hypothesis by the following: two elements always admit of an electrodynamic potential depending solely on their position and orientation; and the work of the forces that they exercise, one on the other, is equal to the variation of this potential. Thus Helmholtz can no more do without hypothesis than Ampère; but at least he does not make one without explicitly announcing it.

- In the case of closed currents, which are alone accessible to experiment, the two theories agree.

In all other cases they differ.

In the first place, contrary to what Ampère supposed, the force which seems to act on the movable portion of a closed current is not the same as would act upon this movable portion if it were isolated and constituted an open current.

Let us return to the circuit C' , of which we spoke above, and

which was formed of a movable wire $\alpha\beta$ sliding on a fixed wire. In the only experiment that can be made, the movable portion $\alpha\beta$ is not isolated, but is part of a closed circuit. When it passes from AB to $A'B'$, the total electrodynamic potential varies for two reasons:

1° It undergoes a first increase because the potential of $A'B'$ with respect to the circuit C is not the same as that of AB ;

2° It takes a second increment because it must be increased by the potentials of the elements AA' , BB' with respect to C .

It is this *double* increment which represents the work of the force to which the portion AB seems subjected.

If, on the contrary, $\alpha\beta$ were isolated, the potential would undergo only the first increase, and this first increment alone would measure the work of the force which acts on AB .

In the second place, there could be no continuous rotation without sliding contact, and, in fact, that, as we have seen *à propos* of closed currents, is an immediate consequence of the existence of an electrodynamic potential.

In Faraday's experiment, if the magnet is fixed and if the part of the current exterior to the magnet runs along a movable wire, that movable part may undergo a continuous rotation. But this does not mean to say that if the contacts of the wire with the magnet were suppressed, and an *open* current were to run along the wire, the wire would still take a movement of continuous rotation.

I have just said in fact that an *isolated* element is not acted upon in the same way as a movable element making part of a closed circuit.

Another difference: The action of a closed solenoid on a closed current is null according to experiment and according to the two theories. Its action on an open current would be null according to Ampère, it would not be null according to Helmholtz. From this follows an important consequence. We have given above three definitions of magnetic force. The third has no meaning here since an element of current is no longer acted upon by a single force. No more has the first any meaning. What, in fact is a magnetic pole? It is the extremity of an indefinite linear magnet. This magnet may be replaced by an indefinite

solenoid. For the definition of magnetic force to have any meaning, it would be necessary that the action exercised by an open current on an indefinite solenoid should depend only on the position of the extremity of this solenoid, that is to say, that the action of a closed solenoid should be null. Now we have just seen that such is not the case.

On the other hand, nothing prevents our adopting the second definition, which is founded on the measurement of the director couple which tends to orientate the magnetic needle.

But if it is adopted, neither the effects of induction nor the electrodynamic effects will depend solely on the distribution of the lines of force in this magnetic field.

III. DIFFICULTIES RAISED BY THESE THEORIES.—The theory of Helmholtz is in advance of that of Ampère; it is necessary, however, that all the difficulties should be smoothed away. In the one as in the other, the word ‘magnetic field’ has no meaning, or, if we give it one, by a more or less artificial convention, the ordinary laws so familiar to all electricians no longer apply; thus the electromotive force induced in a wire is no longer measured by the number of lines of force met by this wire.

And our repugnance does not come alone from the difficulty of renouncing inveterate habits of language and of thought. There is something more. If we do not believe in action at a distance, electrodynamic phenomena must be explained by a modification of the medium. It is precisely this modification that we call ‘magnetic field.’ And then the electrodynamic effects must depend only on this field.

All these difficulties arise from the hypothesis of open currents.

IV. MAXWELL’S THEORY.—Such were the difficulties raised by the dominant theories when Maxwell appeared, who with a stroke of the pen made them all vanish. In his ideas, in fact, there are no longer anything but closed currents. Maxwell assumes that if in a dielectric the electric field happens to vary, this dielectric becomes the seat of a particular phenomenon, acting on the galvanometer like a current, and which he calls *current of displacement*.

If then two conductors bearing contrary charges are put in communication by a wire, in this wire during the discharge there

is an open current of conduction; but there are produced at the same time in the surrounding dielectric, currents of displacement which close this current of conduction.

We know that Maxwell's theory leads to the explanation of optical phenomena, which would be due to extremely rapid electrical oscillation.

At that epoch such a conception was only a bold hypothesis, which could be supported by no experiment.

At the end of twenty years, Maxwell's ideas received the confirmation of experiment. Hertz succeeded in producing systems of electric oscillations which reproduce all the properties of light, and only differ from it by the length of their wave; that is to say as violet differs from red. In some measure he made the synthesis of light.

It might be said that Hertz has not demonstrated directly Maxwell's fundamental idea, the action of the current of displacement on the galvanometer. This is true in a sense. What he has shown in sum is that electromagnetic induction is not propagated instantaneously as was supposed; but with the speed of light.

But to suppose there is no current of displacement, and induction is propagated with the speed of light; or to suppose that the currents of displacement produce effects of induction, and that the induction is propagated instantaneously, *comes to the same thing.*

This can not be seen at the first glance, but it is proved by an analysis of which I must not think of giving even a summary here.

V. ROWLAND'S EXPERIMENT.—But as I have said above, there are two kinds of open conduction currents. There are first the currents of discharge of a condenser or of any conductor whatever.

There are also the cases, in which electric discharges describe a closed contour, being displaced by conduction in one part of the circuit and by convection in the other part.

For open currents of the first sort, the question might be considered as solved; they were closed by the currents of displacement.

For open currents of the second sort, the solution appeared still more simple. It seemed that if the current were closed, it could only be by convection itself. For that it sufficed to admit that a 'convection current,' that is to say a charged conductor in motion, could act on the galvanometer.

But experimental confirmation was lacking. It appeared difficult in fact to obtain a sufficient intensity even by augmenting as much as possible the charge and the velocity of the conductors. It was Rowland, an extremely skillful experimenter, who first triumphed over these difficulties. A disc received a strong electrostatic charge and a very great speed of rotation. An astatic magnetic system placed beside the disc underwent deviations.

The experiment was made twice by Rowland, once in Berlin, once in Baltimore. It was afterward repeated by Himstedt. These physicists even announced that they had succeeded in making quantitative measurements.

In fact, for twenty years Rowland's law was admitted without objection by all physicists. Besides everything seems to confirm it. The spark certainly does produce a magnetic effect. Now does it not seem probable that the discharge by spark is due to particles taken from one of the electrodes and transferred to the other electrode with their charge? Is not the very spectrum of the charge, in which we recognize the lines of the metal of the electrode, a proof of it? The spark would then be a veritable current of convection.

On the other hand, it is also admitted that in an electrolyte, the electricity is carried by the ions in motion. The current in an electrolyte would therefore be also a current of convection; now it acts on the magnetic needle.

And in the same way for cathode rays. Crookes attributed these rays to a very subtile matter charged with electricity and moving with a very great velocity. He regarded them, in other words, as currents of convection. Now these cathode rays are deviated by the magnet. In virtue of the principle of action and reaction, they should in turn deviate the magnetic needle. It is true that Hertz believed he had demonstrated that the cathode rays do not carry electricity, and that they do not act on the magnetic needle. But Hertz was mistaken. First of all, Perrin succeeded in collecting the electricity carried by these rays, electricity of which Hertz denied the existence; the German scientist appears to have been deceived by effects due to the action of X-rays, which were not yet discovered. Afterwards, and quite recently, the action of the cathode rays on the magnetic needle has been put in evidence.

Thus all these phenomena regarded as currents of convection, sparks, electrolytic currents, cathode rays, act in the same manner on the galvanometer and in conformity with Rowland's law.

VI. THEORY OF LORENTZ.—We soon went further. According to the theory of Lorentz, currents of conduction themselves would be true currents of convection. Electricity would remain inseparably connected with certain material particles called electrons. The circulation of these electrons through bodies would produce voltaic currents. And what would distinguish conductors from insulators would be that the one could be traversed by these electrons, while the others could arrest their movements.

The theory of Lorentz is very attractive. It gives a very simple explanation of certain phenomena, which the earlier theories, even Maxwell's, in its primitive form, could only deal with in an unsatisfactory manner. For example, the aberration of light, the partial carrying away of luminous waves, magnetic polarization and the Zeeman effect.

Some objections still remained. The phenomena of an electric system seemed to depend on the absolute velocity of translation of the center of gravity of this system, which is contrary to the idea we have of the relativity of space. Supported by M. Crémieu, M. Lippmann has presented this objection in a striking form. Imagine two charged conductors with the same velocity of translation; they are relatively at rest. However, each of them being equivalent to a current of convection, they ought to attract one another, and by measuring this attraction, we could measure their absolute velocity.

"No!" replied the partisans of Lorentz. "What we could measure in that way is not their absolute velocity, but their relative velocity *with respect to the ether*, so that the principle of relativity is safe."

Whatever there may be in these latter objections, the edifice of electrodynamics, at least in its broad lines, seemed definitely constructed. Everything was presented under the most satisfactory aspect. The theories of Ampère and of Helmholtz, made for open currents which no longer existed, seemed to have no longer anything but a purely historic interest, and the inextricable complications to which these theories led were almost forgotten.

This quiescence has been recently disturbed by the experiments of M. Crémieu, which for a moment seemed to contradict the result previously obtained by Rowland.

But fresh researches have not confirmed them and the theory of Lorentz has victoriously stood the test.

The history of these variations will be none the less instructive; it will teach us to what pitfalls the scientist is exposed, and how he may hope to escape them.

THE END.

APPENDIX.

THE PRINCIPLES OF MATHEMATICAL PHYSICS.*

BY HENRI POINCARÉ.

TRANSLATED BY GEORGE BRUCE HALSTED.

WHAT is the actual state of mathematical physics? What are the problems it is led to set itself? What is its future? Is its orientation on the point of modifying itself?

Will the aim and the methods of this science appear in ten years to our immediate successors in the same light as to ourselves; or, on the contrary, are we about to witness a profound transformation? Such are the questions we are forced to raise in entering to-day upon our investigation.

If it is easy to propound them; to answer is difficult.

If we feel ourselves tempted to risk a prognostication, we have, to resist this temptation, only to think of all the stupidities the most eminent savants of a hundred years ago would have uttered, if some one had asked them what the science of the nineteenth century would be. They would have believed themselves bold in their predictions, and after the event, how very timid we should have found them.

Do not, therefore, expect of me any prophecy; if I had known what will be discovered to-morrow, I should long ago have published it to secure me the priority.

But if, like all prudent physicians, I shun giving a prognosis, nevertheless I can not dispense with a little diagnostic; well, yes, there *are* indications of a serious crisis, as if we might expect an approaching transformation.

We are sure the patient will not die of it, and we may even hope that this crisis will be salutary, that it was even necessary for his development. This the history of the past seems to guarantee us.

* An address delivered before the International Congress of Arts and Science, St. Louis.

This crisis in fact is not the first, and to understand it, it is important to recall those which have preceded it.

Mathematical physics, as we know, was born of celestial mechanics, which engendered it at the end of the eighteenth century, at the moment when it itself attained its complete development. During its first years especially, the infant strikingly resembled its mother.

The astronomic universe is formed of masses, very great, no doubt, but separated by intervals so immense that they appear to us only as material points. These points attract each other inversely as the square of the distance, and this attraction is the sole force which influences their movements. But if our senses were sufficiently subtle to show us all the details of the bodies which the physicist studies, the spectacle we should there discover would scarcely differ from what the astronomer contemplates. There also we should see material points, separated one from another by intervals, enormous in relation to their dimensions, and describing orbits following regular laws.

These infinitesimal stars are the atoms. Like the stars, properly so called, they attract or repel each other, and this attraction or this repulsion following the straight line which joins them, depends only on the distance. The law according to which this force varies as function of the distance is perhaps not the law of Newton, but it is an analogous law; in place of the exponent—2, we have probably a different exponent, and it is from this change of exponent that arises all the diversity of physical phenomena, the variety of qualities and of sensations, all the world colored and sonorous which surrounds us, in a word, all nature.

Such is the primitive conception in all its purity. It only remains to seek in the different cases what value should be given to this exponent in order to explain all the facts. It is on this model that Laplace, for example, constructed his beautiful theory of capillarity; he regards it only as a particular case of attraction, or, as he says, of universal gravitation, and no one is astonished to find it in the middle of one of the five volumes of the '*Mécanique céleste*.'

More recently Briot believed he had penetrated the final secret of optics in demonstrating that the atoms of ether attract each other in the inverse ratio of the sixth power of the distance; and Max-

well, Maxwell himself, does he not say somewhere that the atoms of gases repel each other in the inverse ratio of the fifth power of the distance? We have the exponent — 6, or — 5, in place of the exponent — 2, but it is always an exponent.

Among the theories of this epoch, one alone is an exception, that of Fourier; in it are indeed atoms, acting at a distance one upon the other; they mutually transmit heat, but they do not attract, they never budge. From this point of view, the theory of Fourier must have appeared to the eyes of his contemporaries, to those of Fourier himself, as imperfect and provisional.

This conception was not without grandeur; it was seductive, and many among us have not finally renounced it; they know that one will attain the ultimate elements of things only by patiently disentangling the complicated skein that our senses give us; that it is necessary to advance step by step, neglecting no intermediary; that our fathers were wrong in wishing to skip stations; but they believe that when one shall have arrived at these ultimate elements, there again will be found the majestic simplicity of celestial mechanics.

Neither has this conception been useless; it has rendered us an inestimable service, since it has contributed to make precise in us the fundamental notion of the physical law.

I will explain myself; how did the ancients understand law? It was for them an internal harmony, static, so to say, and immutable; or it was like a model that nature constrained herself to imitate. For us a law is something quite different; it is a constant relation between the phenomenon of to-day and that of to-morrow; in a word, it is a differential equation.

Behold the ideal form of physical law; well, it is the law of Newton which first covered it; and then how has one acclimated this form in physics; precisely by copying as much as possible this law of Newton, that is by imitating celestial mechanics.

Nevertheless, a day arrived when the conception of central forces no longer appeared sufficient, and this is the first of those crises of which I just now spoke.

What was done then? The attempt to penetrate into the detail of the structure of the universe, to isolate the pieces of this vast mechanism, to analyze one by one the forces which put them

in motion, was abandoned, and we were content to take as guides certain general principles the express object of which is to spare us this minute study.

How so? Suppose that we have before us any machine; the initial wheel work and the final wheel work alone are visible, but the transmission, the intermediary wheels by which the movement is communicated from one to the other, are hidden in the interior and escape our view; we do not know whether the communication is made by gearing or by belts, by connecting-rods or by some other arrangement of parts.

Do we say that it is impossible for us to understand anything about this machine so long as we are not permitted to take it to pieces? You know well we do not, and that the principle of the conservation of energy suffices to determine for us the most interesting point. We easily ascertain that the final wheel turns ten times less quickly than the initial wheel; since these two wheels are visible, we are able thence to conclude a couple applied to the one will be balanced by a couple ten times greater applied to the other. For that there is no need to penetrate the mechanism of this equilibrium and to know how the forces compensate each other in the interior of the machine; it suffices to be assured that this compensation can not fail to occur.

Well, in regard to the universe, the principle of the conservation of energy is able to render us the same service. This is also a machine, much more complicated than all those of industry, and of which almost all the parts are profoundly hidden from us; but in observing the movement of those that we can see, we are able, by the aid of this principle, to draw conclusions which remain true whatever may be the details of the invisible mechanism which animates them.

The principle of the conservation of energy, or the principle of Mayer, is certainly the most important, but it is not the only one; there are others from which we are able to draw the same advantage. These are:

The principle of Carnot, or the principle of the degradation of energy.

The principle of Newton, or the principle of the equality of action and reaction.

The principle of relativity, according to which the laws of physical phenomena should be the same, whether for an observer fixed, or for an observer carried along in a uniform movement of translation; so that we have not and could not have any means of discerning whether or not we are carried along in such a motion.

The principle of the conservation of mass, or principle of Lavoisier.

I would add the principle of least action.

The application of these five or six general principles to the different physical phenomena is sufficient for our learning of them what we could reasonably hope to know of them.

The most remarkable example of this new mathematical physics is, beyond question, Maxwell's electromagnetic theory of light.

We know nothing as to what the ether is, how its molecules are disposed, whether they attract or repel each other; but we know that this medium transmits at the same time the optical perturbations and the electrical perturbations; we know that this transmission should be made conformably to the general principles of mechanics, and that suffices us for the establishment of the equations of the electromagnetic field.

These principles are results of experiments boldly generalized; but they seem to derive from their very generality an eminent degree of certitude.

In fact, the more general they are, the more frequently one has the occasion to check them, the more the verifications multiply and take the most varied, the most unexpected forms, the more they end by leaving no further place for doubt.

Such is the second phase of the history of mathematical physics and we have not yet emerged from it.

Shall we say that the first has been useless? that during fifty years science went the wrong way, and that there is nothing left but to forget so many accumulated efforts that a vicious conception condemned in advance to non-success?

Not the least in the world.

Do you believe that the second phase could have come into existence without the first?

The hypothesis of central forces contained all the principles;

it involved them as necessary consequences; it involved both the conservation of energy and that of masses, and the equality of action and reaction, and the law of least action, which would appear, it is true, not as experimental verities, but as theorems, and of which the enunciation would have at the same time a something more precise and less general than under their actual form.

It is the mathematical physics of our fathers which has familiarized us little by little with these diverse principles; which has habituated us to recognize them under the different vestments in which they disguise themselves. One has compared them to the data of experience, or has seen how it was necessary to modify their enunciation to adapt them to these data; thereby they have been enlarged and consolidated.

So one has been led to regard them as experimental verities; the conception of central forces became then a useless support, or rather an embarrassment, since it made the principles partake of its hypothetical character.

The frames have not therefore broken, because they were elastic; but they have enlarged; our fathers, who established them, did not work in vain, and we recognize in the science of to-day the general traits of the sketch which they traced.

Are we about to enter now upon the eve of a second crisis? These principles on which we have built all, are they about to crumble away in their turn? This has been for some time a pertinent question.

In hearing me speak thus, you no doubt think of radium, that grand revolutionist of the present time, and in fact I shall come back to it presently; but there is something else.

It is not alone the conservation of energy which is in question; all the other principles are equally in danger, as we shall see in passing them successively in review.

Let us commence with the principle of Carnot. This is the only one which does not present itself as an immediate consequence of the hypothesis of central forces; more than that, it seems if not to directly contradict that hypothesis, at least not to be reconciled with it without a certain effort.

If physical phenomena were due exclusively to the movements of atoms whose mutual attraction depended only on the distance, it seems that all these phenomena should be reversible; if all the

initial velocities were reversed, these atoms, always subjected to the same forces, ought to go over their trajectories in the contrary sense, just as the earth would describe in the retrograde sense this same elliptic orbit which it describes in the direct sense, if the initial conditions of its movement had been reversed. On this account, if a physical phenomenon is possible, the inverse phenomenon should be equally so, and one should be able to reascend the course of time.

But it is not so in nature, and this is precisely what the principle of Carnot teaches us; heat can pass from the warm body to the cold body; it is impossible afterwards to make it reascend the inverse way and reestablish differences of temperature which have been effaced.

Motion can be wholly dissipated and transformed into heat by friction; the contrary transformation can never be made except in a partial manner.

We have striven to reconcile this apparent contradiction. If the world tends toward uniformity, this is not because its ultimate parts, at first unlike, tend to become less and less different; it is because, shifting at hazard, they end by blending. For an eye which should distinguish all the elements, the variety would remain always as great; each grain of this dust preserves its originality and does not model itself on its neighbors; but as the blend becomes more and more intimate, our gross senses perceive no more than the uniformity. This is why, for example, temperatures tend to a level, without the possibility of turning backwards.

A drop of wine falls into a glass of water; whatever may be the law of the internal movements of the liquid, we soon see it colored of a uniform rosy tint, and from this moment one may well shake the vase, the wine and the water do not seem able any more to separate. See, thus, what would be the type of the reversible physical phenomenon; to hide a grain of barley in a cup of wheat, this is easy; afterwards to find it again and get it out, this is practically impossible.

All this Maxwell and Boltzmann have explained; the one who has seen it most clearly, in a book too little read because it is a little difficult to read, is Gibbs, in his 'Elementary Principles of Statistical Mechanics.'

For those who take this point of view, the principle of Carnot is only an imperfect principle, a sort of concession to the infirmity of our senses; it is because our eyes are too gross that we do not distinguish the elements of the blend; it is because our hands are too gross that we can not force them to separate; the imaginary demon of Maxwell, who is able to sort the molecules one by one, could well constrain the world to return backward. Can it return of itself? That is not impossible; that is only infinitely improbable.

The chances are that we should long await the concurrence of circumstances which would permit a retrogradation; but soon or late they would be realized, after years whose number it would take millions of figures to write.

These reservations, however, all remained theoretic and were not very disquieting, and the principle of Carnot retained all its practical value.

But here the scene changes.

The biologist, armed with his microscope, long ago noticed in his preparations disorderly movements of little particles in suspension; this is the Brownian movement; he first thought this was a vital phenomenon, but soon he saw that the inanimate bodies danced with no less ardor than the others; then he turned the matter over to the physicists. Unhappily, the physicists remained long uninterested in this question; one concentrates the light to illuminate the microscopic preparation, thought they; with light goes heat; thence inequalities of temperature and in the liquid interior currents which produce the movements of which we speak.

It occurred to M. Gouy to look more closely, and he saw, or thought he saw, that this explanation is untenable, that the movements become brisker as the particles are smaller, but that they are not influenced by the mode of illumination.

If then these movements never cease, or rather are reborn without cease, without borrowing anything from an external source of energy, what ought we to believe? To be sure, we should not renounce our belief in the conservation of energy, but we see under our eyes now motion transformed into heat by friction, now heat changed inversely into motion, and that without loss since the movement lasts forever. This is the contrary of the principle of Carnot.

If this be so, to see the world return backward, we no longer have need of the infinitely subtle eye of Maxwell's demon; our microscope suffices us. Bodies too large, those, for example, which are a tenth of a millimeter, are hit from all sides by moving atoms, but they do not budge, because these shocks are very numerous and the law of chance makes them compensate each other: but the smaller particles receive too few shocks for this compensation to take place with certainty and are incessantly knocked about. And behold already one of our principles in peril.

We come to the principle of relativity: this not only is confirmed by daily experience, not only is it a necessary consequence of the hypothesis of central forces, but it is irresistibly imposed upon our good sense, and yet it also is battered.

Consider two electrified bodies; though they seem to us at rest, they are both carried along by the motion of the earth; an electric charge in motion, Rowland has taught us, is equivalent to a current; these two charged bodies are, therefore, equivalent to two parallel currents of the same sense and these two currents should attract each other. In measuring this attraction, we measure the velocity of the earth; not its velocity in relation to the sun or the fixed stars, but its absolute velocity.

I well know what one will say: it is not its absolute velocity that is measured, it is its velocity in relation to the ether. How unsatisfactory that is! Is it not evident that from the principle so understood we could no longer get anything? It could no longer tell us anything just because it would no longer fear any contradiction.

If we succeed in measuring anything, we should always be free to say that this is not the absolute velocity in relation to the ether; it might always be the velocity in relation to some new unknown fluid with which we might fill space.

Indeed, experience has taken on itself to ruin this interpretation of the principle of relativity; all attempts to measure the velocity of the earth in relation to the ether have led to negative results. This time experimental physics has been more faithful to the principle than mathematical physics; the theorists, to put in accord their other general views, would not have spared it; but experiment has been stubborn in confirming it.

The means have been varied in a thousand ways, and finally Michelson has pushed precision to its last limits; nothing has come of it. It is precisely to explain this obstinacy that the mathematicians are forced to-day to employ all their ingenuity.

Their task was not easy, and if Lorentz has got through it, it is only by accumulating hypotheses. The most ingenious idea has been that of local time.

Imagine two observers who wish to adjust their watches by optical signals; they exchange signals, but as they know that the transmission of light is not instantaneous, they take care to cross them.

When the station B perceives the signal from the station A , its clock should not mark the same hour as that of the station A at the moment of sending the signal, but this hour augmented by a constant representing the duration of the transmission. Suppose, for example, that the station A sends its signal when its clock marks the hour 0, and that the station B perceives it when its clock marks the hour t . The clocks are adjusted if the slowness equal to t represents the duration of the transmission, and to verify it, the station B sends in its turn a signal when its clock marks 0; then the station A should perceive it when its clock marks t . The time-pieces are then adjusted. And in fact they mark the same hour at the same physical instant, but on the one condition, that the two stations are fixed. In the contrary case, the duration of the transmission will not be the same in the two senses, since the station A , for example, moves forward to meet the optical perturbation emanating from B , while the station B flies away before the perturbation emanating from A . The watches adjusted in that manner do not mark, therefore, the true time; they mark what one may call the *local time*, so that one of them is slow compared with the other. It matters little, since we have no means of perceiving it. All the phenomena which happen at A , for example, will be late, but all will be equally so, and the observer who ascertains them will not perceive it since his watch is slow; so, as the principle of relativity would have it, he will have no means of knowing whether he is at rest or in absolute motion.

Unhappily, that does not suffice, and complementary hypotheses are necessary; it is necessary to admit that bodies in motion under-

go a uniform contraction in the sense of the motion. One of the diameters of the earth, for example, is shrunk by one two-hundred-millionth in consequence of the motion of our planet, while the other diameter retains its normal length. Thus, the last little differences are compensated. And then, there still is the hypothesis about forces. Forces, whatever be their origin, gravity as well as elasticity, would be reduced in a certain proportion in a world animated by a uniform translation; or, rather, this would happen for the components perpendicular to the translation; the components parallel would not change.

Resume, then, our example of two electrified bodies; these bodies repel each other, but at the same time if all is carried along in a uniform translation, they are equivalent to two parallel currents of the same sense which attract each other. This electrodynamic attraction diminishes, therefore, the electrostatic repulsion, and the total repulsion is feebler than if the two bodies were at rest. But since to measure this repulsion we must balance it by another force, and all these other forces are reduced in the same proportion, we perceive nothing.

Thus, all is arranged, but are all the doubts dissipated?

What would happen if one could communicate by non-luminous signals whose velocity of propagation differed from that of light? If, after having adjusted the watches by the optical procedure, one wished to verify the adjustment by the aid of these new signals, then would appear divergences which would render evident the common translation of the two stations. And are such signals inconceivable, if we admit with Laplace that universal gravitation is transmitted a million times more rapidly than light?

Thus, the principle of relativity has been valiantly defended in these latter times, but the very energy of the defense proves how serious was the attack.

Let us speak now of the principle of Newton, on the equality of action and reaction.

This is intimately bound up with the preceding, and it seems indeed that the fall of the one would involve that of the other. Thus we should not be astonished to find here the same difficulties.

Electrical phenomena, we think, are due to the displacements of little charged particles, called electrons, immersed in the medium

that we call ether. The movements of these electrons produce perturbations in the neighboring ether; these perturbations propagate themselves in every direction with the velocity of light, and in turn other electrons, originally at rest, are made to vibrate when the perturbation reaches the parts of the ether which touch them.

The electrons, therefore, act on one another, but this action is not direct, it is accomplished through the ether as intermediary.

Under these conditions can there be compensation between action and reaction, at least for an observer who should take account only of the movements of matter, that is to say, of the electrons, and who should be ignorant of those of the ether that he could not see? Evidently not. Even if the compensation should be exact, it could not be simultaneous. The perturbation is propagated with a finite velocity; it, therefore, reaches the second electron only when the first has long ago entered upon its rest.

This second electron, therefore, will undergo, after a delay, the action of the first, but certainly it will not react on this, since around this first electron nothing any longer budges.

The analysis of the facts permits us to be still more precise. Imagine, for example, a Hertzian generator, like those employed in wireless telegraphy; it sends out energy in every direction; but we can provide it with a parabolic mirror, as Hertz did with his smallest generators, so as to send all the energy produced in a single direction.

What happens then according to the theory? The apparatus recoils as if it were a gun and as if the energy it has projected were a bullet; and that is contrary to the principle of Newton, since our projectile here has no mass, it is not matter, it is energy.

The case is still the same, moreover, with a beacon light provided with a reflector, since light is nothing but a perturbation of the electromagnetic field. This beacon light should recoil as if the light it sends out were a projectile. What is the force that this recoil should produce? It is what one has called the Maxwell-Bartholdi pressure. It is very minute, and it has been difficult to put into evidence even with the most sensitive radiometers; but it suffices that it exists.

If all the energy issuing from our generator falls on a receiver, this will act as if it had received a mechanical shock, which will

represent in a sense the compensation of the recoil of the generator; the reaction will be equal to the action, but it will not be simultaneous; the receiver will move on, but not at the moment when the generator recoils. If the energy propagates itself indefinitely without encountering a receiver, the compensation will never be made.

Does one say that the space which separates the generator from the receiver and which the perturbation must pass over in going from the one to the other is not void, that it is full not only of ether, but of air; or even in the interplanetary spaces of some fluid subtile but still ponderable; that this matter undergoes the shock like the receiver at the moment when the energy reaches it, and recoils in its turn when the perturbation quits it? That would save the principle of Newton, but that is not true.

If energy in its diffusion remained always attached to some material substratum, then matter in motion would carry along light with it, and Fizeau has demonstrated that it does nothing of the sort, at least for air. This is what Michelson and Morley have since confirmed.

One may suppose also that the movements of matter, properly so called, are exactly compensated by those of the ether; but that would lead us to the same reflections as just now. The principle so extended would explain everything, since, whatever might be the visible movements, we would always have the power of imagining hypothetical movements which compensated them.

But if it is able to explain everything, this is because it does not permit us to foresee anything; it does not enable us to decide between different possible hypotheses, since it explains everything beforehand. It therefore becomes useless.

And then the suppositions that it would be necessary to make on the movements of the ether are not very satisfactory.

If the electric charges double, it would be natural to imagine that the velocities of the diverse atoms of ether double also, and for the compensation, it would be necessary that the mean velocity of the ether quadruple.

This is why I have long thought that these consequences of theory, contrary to the principle of Newton, would end some day by being abandoned, and yet the recent experiments on the movements of the electrons issuing from radium seem rather to confirm them.

I arrive at the principle of Lavoisier on the conservation of masses: certes, this is one not to be touched without unsettling all mechanics.

And now certain persons think that it seems true to us only because in mechanics merely moderate velocities are considered, but that it would cease to be true for bodies animated by velocities comparable to that of light. Now these velocities, it is believed at present, have been realized; the cathode rays or those of radium may be formed of very minute particles or of electrons which are displaced with velocities smaller no doubt than that of light, but which might be its one tenth or one third.

These rays can be deflected, whether by an electric field, or by a magnetic field, and we are able, by comparing these deflections, to measure at the same time the velocity of the electrons and their mass (or rather the relation of their mass to their charge). But when it was seen that these velocities approached that of light, it was decided that a correction was necessary.

These molecules, being electrified, could not be displaced without agitating the ether; to put them in motion it is necessary to overcome a double inertia, that of the molecule itself and that of the ether. The total or apparent mass that one measures is composed, therefore, of two parts: the real or mechanical mass of the molecule and the electrodynamic mass representing the inertia of the ether.

The calculations of Abraham and the experiments of Kaufmann have then shown that the mechanical mass, properly so called, is null, and that the mass of the electrons, or, at least, of the negative electrons, is of exclusively electrodynamic origin. This forces us to change the definition of mass; we can not any longer distinguish mechanical mass and electrodynamic mass, since then the first would vanish; there is no mass other than electrodynamic inertia. But, in this case the mass can no longer be constant; it augments with the velocity, and it even depends on the direction, and a body animated by a notable velocity will not oppose the same inertia to the forces which tend to deflect it from its route, as to those which tend to accelerate or to retard its progress.

There is still a resource; the ultimate elements of bodies are electrons, some charged negatively, the others charged positively.

The negative electrons have no mass, this is understood; but the positive electrons, from the little we know of them, seem much greater. Perhaps, they have, besides their electrodynamic mass, a true mechanical mass. The real mass of a body would, then, be the sum of the mechanical masses of its positive electrons, the negative electrons not counting; mass so defined might still be constant.

Alas! this resource also evades us. Recall what we have said of the principle of relativity and of the efforts made to save it. And it is not merely a principle which it is a question of saving, such are the indubitable results of the experiments of Michelson.

Lorentz has been obliged to suppose that all forces, whatever be their origin, were affected with a coefficient in a medium animated by a uniform translation; this is not sufficient; it is still necessary, says he, that *the masses of all the particles be influenced by a translation to the same degree as the electromagnetic masses of the electrons.*

So the mechanical masses will vary in accordance with the same laws as the electrodynamic masses; they can not, therefore, be constant.

Need I point out that the fall of the principle of Lavoisier involves that of the principle of Newton? This latter signifies that the center of gravity of an isolated system moves in a straight line; but if there is no longer a constant mass, there is no longer a center of gravity, we no longer know even what this is. This is why I said above that the experiments on the cathode rays appeared to justify the doubts of Lorentz on the subject of the principle of Newton.

From all these results, if they are confirmed, would arise an entirely new mechanics, which would be, above all, characterized by this fact, that no velocity could surpass that of light, any more than any temperature could fall below the zero absolute, because bodies would oppose an increasing inertia to the causes which would tend to accelerate their motion; and this inertia would become infinite when one approached the velocity of light.

No more for an observer, carried along himself in a translation he did not suspect, could any apparent velocity surpass that of light; and this would be then a contradiction, if we recall that

this observer would not use the same clocks as a fixed observer, but, indeed, clocks marking 'local time.'

Here we are then facing a question I content myself with stating. If there is no longer any mass, what becomes of the law of Newton?

Mass has two aspects: it is at the same time a coefficient of inertia and an attracting mass entering as factor into Newtonian attraction. If the coefficient of inertia is not constant, can the attracting mass be? That is the question.

At least, the principle of the conservation of energy yet remains to us, and this seems more solid. Shall I recall to you how it was in its turn thrown into discredit? This event has made more noise than the preceding, and it is in all the memoirs.

From the first works of Becquerel, and, above all, when the Curies had discovered radium, it was seen that every radioactive body was an inexhaustible source of radiations. Its activity would seem to subsist without alteration throughout the months and the years. This was in itself a strain on the principles; these radiations were in fact energy, and from the same morsel of radium this issued and forever issued. But these quantities of energy were too slight to be measured; at least one believed so and was not much disquieted.

The scene changed when Curie bethought himself to put radium in a calorimeter; it was then seen that the quantity of heat incessantly created was very notable.

The explanations proposed were numerous; but in such case we can not say, 'store is no sore.'

In so far as no one of them has prevailed over the others, we can not be sure there is a good one among them.

Sir W. Ramsay has striven to show that radium is in process of transformation, that it contains a store of energy enormous but not inexhaustible.

The transformation of radium then would produce a million times more heat than all known transformations; radium would wear itself out in 1,250 years; you see that we are at least certain to have this point settled some hundreds of years from now. While waiting, our doubts remain.

In the midst of so many ruins what remains standing? The

principle of least action is hitherto intact, and Larmor appears to believe that it will long survive the others; in reality, it is still more vague and more general.

In presence of this general ruin of the principles, what attitude will mathematical physics take?

And first, before too much excitement, it is proper to ask if all that is really true. All these derogations to the principles are encountered only among infinitesimals; the microscope is necessary to see the Brownian movement; electrons are very light; radium is very rare, and one never has more than some milligrams of it at a time.

And then it may be asked if, besides the infinitesimal seen, there be not another infinitesimal unseen counterpoise to the first.

So, there is an interlocutory question, and, as it seems, only experiment can solve it. We have, therefore, only to hand over the matter to the experimenters, and while waiting for them to finally decide the debate, not to preoccupy ourselves with these disquieting problems, and to tranquilly continue our work, as if the principles were still uncontested. Certes, we have much to do without leaving the domain where they may be applied in all security; we have enough to employ our activity during this period of doubts.

And as to these doubts, is it indeed true that we can do nothing to disembarass science of them? It may be said, it is not alone experimental physics that has given birth to them; mathematical physics has well contributed. It is the experimenters who have seen radium throw out energy, but it is the theorists who have put in evidence all the difficulties raised by the propagation of light across a medium in motion; but for these it is probable we should not have become conscious of them. Well, then, if they have done their best to put us into this embarrassment, it is proper also that they help us to get out of it.

They must subject to critical examination all these new views I have just outlined before you, and abandon the principles only after having made a loyal effort to save them.

What can they do in this sense? That is what I will try to explain.

Among the most interesting problems of mathematical physics, it is proper to give a special place to those relating to the kinetic

theory of gases. Much has already been done in this direction, but much still remains to be done. This theory is an eternal paradox. We have reversibility in the premises and irreversibility in the conclusions; and between the two an abyss. Do statistic considerations, the law of great numbers, suffice to fill it? Many points still remain obscure to which it is necessary to return, and doubtless many times. In clearing them up, we shall understand better the sense of the principle of Carnot and its place in the ensemble of dynamics, and we shall be better armed to properly interpret the curious experiment of Gouy, of which I spoke above.

Should we not also endeavor to obtain a more satisfactory theory of the electrodynamics of bodies in motion? It is there especially, as I have sufficiently shown above, that difficulties accumulate. Evidently we must heap up hypotheses, we can not satisfy all the principles at once; heretofore, one has succeeded in safeguarding some only on condition of sacrificing the others; but all hope of obtaining better results is not yet lost. Let us take, therefore, the theory of Lorentz, turn it in all senses, modify it little by little, and perhaps everything will arrange itself.

Thus in place of supposing that bodies in motion undergo a contraction in the sense of the motion, and that this contraction is the same whatever be the nature of these bodies and the forces to which they are otherwise submitted, could we not make a more simple and natural hypothesis?

We might imagine, for example, that it is the ether which is modified when it is in relative motion in reference to the material medium which it penetrates, that when it is thus modified, it no longer transmits perturbations with the same velocity in every direction. It might transmit more rapidly those which are propagated parallel to the medium, whether in the same sense or in the opposite sense, and less rapidly those which are propagated perpendicularly. The wave surfaces would no longer be spheres, but ellipsoids, and we could dispense with that extraordinary contraction of all bodies.

I cite this only as an example, since the modifications, one might essay, would be evidently susceptible of infinite variation.

It is possible also that astronomy may some day furnish us data on this point; she it was in the main who raised the question in

making us acquainted with the phenomenon of the aberration of light. If we make crudely the theory of aberration, we reach a very curious result. The apparent positions of the stars differ from their real positions because of the motion of the earth, and as this motion is variable, these apparent positions vary. The real position we can not know, but we can observe the variations of the apparent position. The observations of the aberration show us, therefore, not the movement of the earth, but the variations of this movement; they can not, therefore, give us information about the absolute motion of the earth. At least this is true in first approximation, but the case would be no longer the same if we could appreciate the thousandths of a second. Then it would be seen that the amplitude of the oscillation depends not alone on the variation of the motion, a variation which is well known, since it is the motion of our globe on its elliptic orbit, but on the mean value of this motion; so that the constant of aberration would not be altogether the same for all the stars, and the differences would tell us the absolute motion of the earth in space.

This, then, would be, under another form, the ruin of the principle of relativity. We are far, it is true, from appreciating the thousandths of a second, but, after all, say some, the total absolute velocity of the earth may be much greater than its relative velocity with respect to the sun. If, for example, it were 300 kilometers per second in place of 30, this would suffice to make the phenomena observable.

I believe that in reasoning thus one admits a too simple theory of aberration. Michelson has shown us, I have told you, that the physical procedures are powerless to put in evidence absolute motion; I am persuaded that the same will be true of the astronomic procedures, however far one pushes precision.

However that may be, the data astronomy will furnish us in this regard will some day be precious to the physicist. While waiting, I believe, the theorists, recalling the experience of Michelson, may anticipate a negative result, and that they would accomplish a useful work in constructing a theory of aberration which would explain this in advance.

But let us come back to the earth. There also we may aid the experimenters. We can, for example, prepare the ground by study-

ing profoundly the dynamics of electrons; not, be it understood, in starting from a single hypothesis, but in multiplying hypotheses as much as possible. It will be then for the physicists to utilize our work in seeking the crucial experiment to decide between these different hypotheses.

This dynamics of electrons can be approached from many sides, but among the ways leading thither is one which has been somewhat neglected, and yet this is one of those which promise us most of surprises. It is movements of the electrons which produce the line of the emission spectra; this is proved by the Zeemann effect; in an incandescent body, what vibrates is sensitive to the magnet, therefore electrified. This is a very important first point, but no one has gone farther. Why are the lines of the spectrum distributed in accordance with a regular law?

These laws have been studied by the experimenters in their least details; they are very precise and relatively simple. The first study of these distributions recalled the harmonics encountered in acoustics; but the difference is great. Not only the numbers of vibrations are not the successive multiples of one same number, but we do not even find anything analogous to the roots of those transcendental equations to which so many problems of mathematical physics conduct us: that of the vibrations of an elastic body of any form, that of the Hertzian oscillations in a generator of any form, the problem of Fourier for the cooling of a solid body.

The laws are simpler, but they are of wholly other nature, and to cite only one of these differences, for the harmonics of high order the number of vibrations tends toward a finite limit, instead of increasing indefinitely.

That has not yet been accounted for, and I believe that there we have one of the most important secrets of nature. Lindemann has made a praiseworthy attempt, but, to my mind, without success; this attempt should be renewed. Thus we will penetrate, so to say, into the inmost recess of matter. And from the particular point of view which we to-day occupy, when we know why the vibrations of incandescent bodies differ from ordinary elastic vibrations, why the electrons do not behave themselves like the matter which is familiar to us, we shall better comprehend the dynamics of electrons and it will be perhaps more easy for us to reconcile it with the principles.

Suppose, now, that all these efforts fail, and, after all, I do not believe they will, what must be done? Will it be necessary to seek to mend the broken principles in giving what we French call a *coup de pousse*? That is evidently always possible, and I retract nothing I have formerly said.

Have you not written, you might say if you wished to seek a quarrel with me—have you not written that the principles, though of experimental origin, are now unassailable by experiment because they have become conventions? And now you have just told us the most recent conquests of experiment put these principles in danger. Well, formerly I was right and to-day I am not wrong.

Formerly I was right, and what is now happening is a new proof of it. Take, for example, the calorimeter experiment of Curie on radium. Is it possible to reconcile that with the principle of the conservation of energy?

It has been attempted in many ways; but there is among them one I should like you to notice.

It has been conjectured that radium was only an intermediary, that it only stored radiations of unknown nature which flashed through space in every direction, traversing all bodies, save radium, without being altered by this passage and without exercising any action upon them. Radium alone took from them a little of their energy and afterward gave it out to us in diverse forms.

What an advantageous explanation, and how convenient! First, it is unverifiable and thus irrefutable. Then again it will serve to account for any derogation whatever to the principle of Mayer; it replies in advance not only to the objection of Curie, but to all the objections that future experimenters might accumulate. This energy new and unknown would serve for everything. This is just what I have said, and therewith we are shown that our principle is unassailable by experiment.

And, after all, what have we gained by this *coup de pousse*?

The principle is intact, but thenceforth of what use is it?

It permitted us to foresee that in such or such circumstance we could count on such a total quantity of energy; it limited us; but now that this indefinite provision of new energy is placed at our disposal, we are limited by nothing; and, as I have written also, if a principle ceases to be fecund, experiment without contradicting it directly, will nevertheless have condemned it.

This, therefore, is not what would have to be done; it would be necessary to rebuild anew.

If we were reduced to this necessity, we should moreover console ourselves. It would not be necessary thence to conclude that science can weave only a Penelope's web, that it can build only ephemeral constructions, which it is soon forced to demolish from top to bottom with its own hands.

As I have said, we have already passed through a like crisis. I have shown you that in the second mathematical physics, that of the principles, we find traces of the first, that of the central forces; it will be just the same if we must learn a third.

Of such an animal as exuviates, as breaks its too narrow carapace and makes itself a fresh one, under the new envelope we easily recognize the essential traits of the organism which have subsisted.

We can not foresee in what way we are about to expand; perhaps it is the kinetic theory of gases which is about to undergo development and serve as model to the others. Then, the facts which first appeared to us as simple, thereafter will be merely results of a very great number of elementary facts which only the laws of chance make cooperate for a common end. Physical law will then assume an entirely new aspect; it will no longer be solely a differential equation, it will take the character of a statistical law.

Perhaps, likewise, we should construct a whole new mechanics, that we only succeed in catching a glimpse of, where inertia increasing with the velocity, the velocity of light would become an impassable limit.

The ordinary mechanics, more simple, would remain a first approximation, since it would be true for velocities not too great, so that one would still find the old dynamics under the new.

We should not have to regret having believed in the principles, and even, since velocities too great for the old formulas would always be only exceptional, the surest way in practice would be still to act as if we continued to believe in them. They are so useful, it would be necessary to keep a place for them. To determine to exclude them altogether, would be to deprive oneself of a precious weapon. I hasten to say in conclusion we are not yet there, and as yet nothing proves that the principles will not come forth from the combat victorious and intact.

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